The JOURNAL of SOCIAL ISSUES

Vol. VI No. 4

1950

DETROIT PUBLIC LIBRARY

Values and the Social Scientist

Issue Editors: KENNETH D. BENNE and G. E. SWANSON

Contents of Issue

The Problem of Values and the Social Scientist.....Issue Editors— 2

Values and Social Science .....George Geiger— 8

A Symposium of Commentaries by: C. E. Ayres, Raymond B. Cattell, Franz Alexander, Henry N. Wieman, Herbert Feigl, R. Bruce Raup, Ronald Lippitt, Nicholas Hobbs, C. West Churchman—17

Rejoinder .....George Geiger—64

A Selective Summary in Process.....Issue Editors—69

Volume VI **1950** **Number 4**

THE JOURNAL OF SOCIAL ISSUES

Published Quarterly by the Association Press for

THE SOCIETY FOR THE PSYCHOLOGICAL STUDY OF SOCIAL ISSUES

A Division of

THE AMERICAN PSYCHOLOGICAL ASSOCIATION

Annual Subscription: $3.00 Single Copy $1.00

(For group rates and back numbers see last page)

STATEMENT OF PURPOSE

The SPSSI is a group of several hundred social psychologists and allied social scientists with a particular interest in research on the psychological aspects of key social issues. The Society believes it has an obligation to help get scientific knowledge into the bloodstream of applied social action by building as many bridges as possible between theory and practice in group, community, national, and international living. This Journal has as its goal the communication of scientific findings and interpretations in a non-technical manner but without the sacrifice of professional standards.

Business address of publisher: Subscriptions, orders, and business communications should be sent to ASSOCIATION PRESS, 291 Broadway, New York 7, N.Y.

Editorial address: Suggestions and inquiries concerning editorial content should be sent to: Ronald Lippitt, Research Center for Group Dynamics, University of Michigan, Ann Arbor, Michigan.

Society address: Prof. S. Stansfeld Sargent, Secretary-Treasurer, Psychology Department, Barnard College, Columbia University, New York 27, New York.

Executive Secretary: Requests for information about the Society and applications for membership should be sent to Dr. Otto Klineberg, Psychology Department, Columbia University, New York 27, New York.

Executive Editorial Committee: Robert Angell, Roger Heyns, Max Hutt, George Katona, Harold Kelley, Jacob Kounin, Alfred Lee, Ronald Lippitt, Eleanor Maccoby, Norman R. F. Maier, Horace Miner, Theodore Newcomb.

Advisory Editorial Board: Robert F. Bales, Melvin W. Barnes, Urie Bronfenbrenner, Isidor Chein, Fred Couey, Kermit Eby, James H. Elder, Lawrence K. Hall, Robert R. Holt, Lloyd G. Humphreys, David Krech, Robert K. Merton, Herbert A. Thelen, Ruth S. Tolman, Ralph K. White.

Printed by

Ann Arbor Press, 317 Maynard St., Ann Arbor, Mich.

PREFACE

The editors, Professor Geiger, and the other contributors have performed a very real service for all of us in this issue. If you are a social scientist you will find much stimulation here for reflection on your own scientific activities and on your role in society. If you are a professional practicioner—an educator, social worker, public administrator, doctor, etc. —you will have an opportunity to analyze some of the crucial problems of relationship between scientific research and your professional practice as an agent of social welfare and social change. If you are a citizen layman you have a very strong vested interest in this discussion. Does it reflect a growing interaction between scientific discovery and the crucial social problems you are grappling with? Or is there a wide cleavage between social science and practical action? Is there any evidence, as some claim, that social scientists are likely to acquire an undemocratic control of our lives? Or do we need to foster the training and activity of more social scientists?

In many ways this is perhaps the most important social issue we have dealt with in the six years of publication. Not because it is creating such widespread social disruption, but because it represents such a dynamic and underlying potential. I hope the interactions between the contributors may be the starting point for a variety of chain reactions.

RONALD LIPPITT,
General Editor.

A Final Note—For six years the readers of this journal have been dependent on the selective enthusiasms and biases of the same editor. The editorial term policy of the Society for the Psychological Study of Social Issues ensures the readers of a well deserved change at this point. The members of the Society and all other readers are very fortunate in having the services of Professor Brewster Smith, head of the Department of Psychology of Vassar College, as the new editor. He has a broad, insightful, and eclectic orientation to the fields of social science and social practice. The journal will continue its policy of bridging the gap between science and action, and will continue to be a channel for collaborative publication of research reports and interpretation on topics of social significance. The retiring editor has profited greatly by the opportunity for stimulation from so many colleagues. He has also been greatly encouraged by the evidence of growing usefulness of the journal to an increasing number and variety of readers. This usefulness will certainly continue to grow under the fresh stimulus of our new editor. *R. L.*

THE PROBLEM OF VALUES AND THE SOCIAL SCIENTIST

KENNETH D. BENNE AND G. E. SWANSON

IN THIS number of the *Journal,* social scientists from several academic disciplines, philosophers of science and education, and a theologian carry on a forum discussion concerning the *bases* upon which the student of human life may (and should) ground the choices and decisions which of necessity he must make. The high ratio of scientists to theologians among the contributors might lead the reader to look upon this effort as heir of the 18th century's Enlightenment. And there is among many of the contributors, not least the theologian, an "18th century hope" that a "science of values" is possible, if not well on its way to development. But there is here little of the Enlightenment's tendency to elevate the scientist above merely moral men, men beset with questions of good and evil, or its accompanying tendency to hope that out of the scientific "objectivity" so achieved superior wisdom would somehow develop, wisdom which would render men's traditional problems of choice obsolete or perhaps furnish obvious, enlightened answers to these. There is here rather a tendency to accept the fact that scientists are human beings and the scientific enterprise a human enterprise. As human beings, scientists are inescapably bound to engage in the most distinctively human undertaking of all, the making of choices, with all of the pains and uncertainties which typically attend the making. If scientists are to contribute to a science of values, so most contributors seem to agree, they must become committed students of human choices, not least of their own. In their admission that scientists too are human, and in the sober cautiousness of the generalizations proffered concerning the valid *bases* of human decisions, these papers are unmistakably the products of the 20th century's Disenchantment.

The human story which has led men and scientists from Enlightenment to Disenchantment, part of which is suggested in the papers in this symposium, tells us something about modern social science but perhaps more about social scientists as modern men. As several contributors point out, social scientists have in the past frequently tried to divorce their craft and its exercise from values and value judgments. They have hoped that concentration upon the collection of data and objective interpretation of these would permit them to forget the valuative behavior involved in the methods by which problems and topics are chosen for study, by which the relevance and importance of the data to be collected are judged, by which interpretations are formulated and communicated. Yet, as social science has come to be seen as a human enterprise, although a special one, the inescapability of the choices involved in its exercise has come home to more and more social scientists. Choices always involve standards of choice, explicit or implicit or both. If we can not escape the making of choices, many social scientists have come to ask, how can we improve the

bases upon which (the methods by which) our choices get made; if indeed "improvement" is capable of definition not to mention achievement?

The passion to decontaminate science from contamination with values was not, in its heyday, merely the expression of some ritualistic need by scientists for cold, hard data. Every scientist knew how easily and how dangerously his personal hopes and anxieties might enter the research process to reduce the validity and the reliability of the research product. He wanted to keep the "human equation" away from his findings. If data could only be made to speak for themselves, all would be well. And it is understandable that social scientists, with their lesser ability, compared with that of physical scientists, to make crucial and controlled tests of their hypotheses, should be especially sensitive to the dangers of choices and choosing.

By the beginning of the 20th century, serious students of science as a human enterprise had come to admit that choices were unavoidable in the research processes by which knowledge is produced. And both "personal" and "social" interests and outlooks had been identified as inescapable influences upon those choices.

It had come to be freely admitted that problems for research are always chosen by the scientist, and it was confessed, often ruefully, that such choices were usually made on irrational grounds. It was also admitted, though less willingly, that the interpretations made by scientists were not dictated by the data speaking pure truth to objective scholarly ears but by an active selecting, weighing, and summarizing on the part of the investigator. Each of these steps in research involved an act of choice and the operation of criteria of choosing in the process.

Aside from its possible merits as a statement of the actual situation faced by the practicing scientist, this discussion had led to other results. It had helped to make a part of the working knowledge of large numbers of scientists the recognition that the assumptions underlying the method of science itself and the whole gamut of steps in the application of that method involved choices and, hence, values. It had also crystallized a latent distinction in scientific thinking—a distinction between choice situations in which the individual made as rational a search as he could for that course, among many, which would bring him closest to his goals and choice situations in which little of such considered judgment was exercised. This distinction had long existed in the canons of science. Now it appeared that, while the inescapable choices by the scientist might actually be of either sort, the former kind of choosing might well be the more effective for reaching scientific goals. Much of later thinking about the scientific enterprise has elaborated on the meanings of this insight.

One line of elaboration took form as a study of the social origins of knowledge. In the writings of Mannheim,[1] Marx,[2, 3] Dilthey,[4] Scheler [5] and others, this study detailed the subtle ways in which even seemingly irrelevant values held by researchers entered into their work. It resulted in the dictum that the freeing of facts from fancies required two actions

by scientists: 1) the fullest possible cross-checking of research methods, findings and interpretations by scholars with widely different views; and 2) the most explicit statements scientists could make about the preconceptions they felt had influenced their own work. This personal and collective scientific soul-searching came to be commended to the body scientific as a standard part of research technique.

A second elaboration of the means for making effective choices, both by scientists and by other men, came from the work of John Dewey and his colleagues.[6, 7, 8, 9, 10] For American pragmatists, the good came to be sought in the continuum of ends and means. One of several competing ends was to be chosen in terms of its own comparative means-value in the service of still other ends. This turned the focus of ethics away from pre-formed and codified decisions assumed to be valid for all occasions and directed attention of moralists and other value theorists toward methods of problem solving having the highest and most general usefulness for satisfying human desires, including the desire for verified knowledge. The method of science, properly generalized and humanized, came to be held as the most direct and dependable path to the good as well as the true.

These two elaborations had much in common. Both assumed that the source of values is the human situation; both asserted that values are functional instruments used and shaped in the processes of human choosing and that they do and should change as human needs and the human environment change; and both rejected the validity of any fixed and final list of standard decisions for guiding value judgments—except perhaps the standards implicit in the method of science itself.

It is also certain that these elaborations were by no means identical. The sociology of knowledge, influenced by studies of comparative culture, has suggested that the scientist is forever and forever biased by cultural perspectives and can hope to escape those biases only by acquiring new and

---

[1] Mannheim, Karl, *Ideology and Utopia,* New York, Harcourt, Brace and Co., 1940.

[2] Marx, Karl and Engels, Friederich, "The Communist Manifesto," (In Burns, Emile *ed. A Handbook of Marxism,* New York, Random House, 1935.)

[3] Marx, Karl and Engels, Friederich, "Die Deutsche Ideologie," (In Adoratsky, V., *ed. Historisch-Kritische Gesamtausgabe, Werke, Schriften, Briefe,* Berlin, Marx-Engels-Lenin Verlag, g.m.b.h., bd. 5.)

[4] Among the more available discussions of Dilthey in English are: Abel, Theodore, *Systematic Sociology in Germany,* New York, Columbia University Press, 1929; and the article on Dilthey's work in Seligman, Edwin R. A., ed.-in-chief, *Encyclopedia of the Social Sciences,* New York, The MacMillan Co., 1934, Vol. XII, p. 213.

[5] A good source in English on the contributions of Scheler is: Mannheim, Karl, *op. cit.,* pp. 158, 242, 251, 268, 279.

[6] Dewey, John, *Studies in Logical Theory,* Chicago, The University of Chicago Press, 1903. (The Decennial Publications, 2d series, Vol. XI.)

[7] Dewey, John, *Logical Conditions of a Scientific Treatment of Morality,* Chicago, The University of Chicago Press, 1903. (First series, Vol. 3, Decennial Publications of the University of Chicago, pp. 115–139.)

[8] Dewey, John, *Human Nature and Conduct,* New York, Henry Holt and Co., 1922.

[9] Dewey, John, *The Public and Its Problems,* New York, Henry Holt and Co., 1927.

[10] Dewey, John, *Theory of Valuation,* Chicago, University of Chicago Press, 1939. (In the International Encyclopedia of Unified Science, Vol. III, No. 4.)

different ones, with the result that he is never certain of being closer to final good or truth than he was before. American pragmatism has seemed to affirm the possibility of man's improving his power to predict events and to control them for the fuller realization of common purposes, even across the deepest cleavages in cultural perspective.

One further characteristic of these elaborations has importance here. In all known societies, decision-making gains direction from being pointed toward and guided by certain core values, which for that "society" at least transcend all others. Neither the sociology of knowledge nor pragmatic philosophy provides such goals nor prescribes unambiguously how men, confronted by cultural crisis in which core values tend to disintegrate, go about rebuilding and securing such values. Pragmatism does suggest methods for finding them, but to many seeking a confident core of common values which transcends current conflicts, it seems to offer little encouragement that they will be found. To many, it seems to say only—If common goals are found by the use of our method, they will be more satisfying to the seekers than would those arrived at by less considered means.

The common emphases of the two principal elaborations of our new sophistication concerning the inseparability of valuation and the scientific enterprise, outlined just above, may or may not be correct. It is as certainly true that they are held as correct by many members of the social science fraternity and by many philosophers of science as it is that vigorous objections to some or all of these emphases are currently raised both by social scientists and by philosophers. Documentation for both these "certainties" is to be found within this symposium. Part of the test of adequacy for any interpretation of the relationships between values and the social sciences is its bearing on certain related yet distinct "extra-scientific" problems. What would be the effect of this interpretation or that upon the stability of individuals or upon the persistence of human groups in contemporary society? What is the likelihood that this interpretation or that will be widely or readily accepted by scientists or by other men in our society? How does this interpretation or that contribute to the social and psychological prerequisites for consistent value-behavior on the part of scientist and citizen? The present symposium shows no clear-cut consensus concerning the solution of these related problems and the reader will have for the most part to apply this suggested test for himself.

At this point in the mid-twentieth century, with the problem of the nature and basis of valid choice still unresolved for social scientists (as for other men), and with the criteria for valid choice still in doubt in the thinking of many of them, this forum has been arranged in order to clarify the issues involved and perhaps to add new insights. A philosopher of science, with a well-developed pragmatic position on the interrelation of "science" and "values," Professor George Geiger of Antioch College, was invited to contribute a summary of his position. Other contributors were asked to use this summary as a point of departure for a presentation

of their own views. And Professor Geiger was given the privilege of a rejoinder to these. Each contributor was asked to make clear the bearings of his position "upon the work and responsibility of the social scientist."

However well the contributors, within their rather rigid assigned word limits, may have been able to meet this last request, it seems certain that the responsibility for judgment concerning the bearings of various positions upon the choices which the social scientist must make in his work rests with the readers who are also social scientists. In order to facilitate this process of judgment on the part of readers, the Editors have undertaken the following analysis of the choices which must be made by social scientists in their work. These problems of valuation for the social scientist have been grouped for convenience around the three principal roles which the contemporary social scientist is called upon to play—the role of researcher, the role of expert consultant, and the role of citizen.

Value Problems of the Scientist as Researcher

These problems are very old in the history of science, however recent may have been their explicit recognition as problems by social scientists.

The social scientist must choose:

1. The problems he will study;
2. The methods he will use for motivating, manipulating, and rewarding the people he studies;
3. The interpretations he makes of the data collected;
4. The place, time, and manner of publication;
5 The relationships, if any, he will draw between his work and that of his colleagues;
6. The kind of professional discipline by which he will be bound and which he is willing to help enforce upon others;
7. The relationships he will establish with colleagues as members of a reasearch team, as a professional audience, and as a group having a professional vested interest in his work and conduct.

Value Problems of the Scientist as an Expert Consultant

These problems are not actually new (they have always operated, for example, in the work of social scientist as teacher, though teaching has not typically been seen in this way). But the frequency with which they appear, and the number of scientists touched by them, seem to be growing rapidly. This growth has accompanied the changing role of social science in society—especially perhaps the roles of the "younger" disciplines of psychology, sociology, and cultural anthropology. Only in the last two or three decades have these disciplines found themselves called upon extensively by policy-makers in industry, labor, government, education, social work, and public health for advice and policy-oriented research. Whether these appeals for help arise from the accumulating needs for social engineering in an increasingly complex society, or from a rise in the population's internal tensions, or from the problems of institutional

adjustment to persistent and rapid change, or from some combination of these, there is currently an upswing in the demand for social scientists as consultants.

The community wants more than their consultation as individuals. It wants consultation by advisers of publicly recognized competence. Part of the guarantee of such competence lies in the establishment and maintenance of professional standards. Like the older consulting professions of law, medicine, and the ministry, social scientists are now compelled to develop and enforce such an occupational value system as a professional group or have it imposed on them.

Problems here include the determination of criteria by which the consultant decides:

1. Whether or not he will accept clients wanting advice or data and whether or not he will consult with organizations and groups having a certain ideological orientation and not with others;
2. The kinds of work he will do for clients;
3. The designing, conduct, and interpretations of that work, including how much he shares these operations with the client;
4. The limitations he will accept upon the use and publication of research findings by the employing group.

Value Problems of the Scientist as a Citizen

For many social scientists, the fact of having knowledge and skills which are of immediate usefulness for social action sets difficult problems of decision. Among these are adjustments about:

1. Whether or not to use his skills and knowledge for facilitating social action;
2. Whether to identify himself with certain social action groups, thus perhaps reducing his "freedom of movement" as scientist and consultant;
3. The ethical limitations if any upon methods of using his skills and knowledge in social action;
4. The stand to take vis-a-vis groups that want to limit the uses which he and others may choose to make of their knowledge and skills.

Of course there are social scientists for whom these decisions are already made. For some the solution is a solution by default—they do not and perhaps can not recognize these decisions as presenting value problems. For others, and this group probably includes a larger number, decisions seem to get made for them as the culture of their society and the traditions of their profession determine them. Some may find such outside determination a convenient, if not necessarily rational, way of handling the matter. It may be unethical, but it saves time and effort of choice. But probably most of us are genuinely puzzled and anxious for help in developing more rational ways of defining and solving our value problems.

So let us join Dr. Geiger and his discussants. They may have something to offer us in our need.

VALUES AND SOCIAL SCIENCES

GEORGE GEIGER

MORE than anyone else, the social scientist should be sensitive to the phenomenon of cultural lag. As a matter of fact, the critical analysis of ceremonialisms and "pseudomorphs" does enlist a considerable share of his energies. It is a little surprising, then, that one of the most conspicuous (and mischievous) cultural hang-overs still plagues social science, just as it haunts natural science and philosophy. I refer to the antique dualism between fact and value. To be sure, the insulation is now being worn a little thin as "Atom" and "Hydrogen" begin to exert their pressure, and as presidents of learned societies and public figures deplore daily the unequal race between technology and morals; but, despite the spate of testimonials, even of confessions, the institutionalized separation of descriptive inquiry from normative inquiry continues to be respectable, academic, and stultifying.

One basic assumption, therefore, needs to be made clear at the start, and that is that the alleged fact-value dualism has already been pierced by contemporary events. These first five years of the Atom-Hydrogen Age have already made anachronistic the traditional compartmentalizing of human experience into what we do and what we ought to do. Not that something essentially novel has been introduced by the drama of released nuclear energy: the problem of the differential lag between knowledge and action has been a burning one since at least the days of the Hebrew prophets. But now it has been given an incandescence which can escape neither journalist nor philosopher, scientist nor layman.

Another assumption can be added, one which will direct the course of the entire argument: that in this question of a scientific approach to value the whole burden of proof has been misplaced. The characteristic assumption, in much of both science and philosophy, has been that human evaluations are automatically outside the ambit of scientific method, and that the burden of proof rested on those who saw ethics as a candidate for scientific consideration. I suggest that this burden can and must be shifted. This suggestion involves the opening reference to cultural lag, for the contention here is that the reason for separating values from intelligent inquiry has been social and institutional, not theoretical. That is to say, there is nothing intrinsic in the nature of value which puts it forever beyond the pale. Indeed, the very question as to the commerce between ethics and science should never have been raised, not, at least, in that familiar and threatening form which has proved such a bogey to conscientious men.

These statements are being made abruptly and will have to be justified presently. Before that, it would appear that any philosophic analysis of a problem like this should start not only with assumptions but with defi-

nitions. After all, what is meant by "value," and what by "scientific method"?

It may seem plausible to start with definitions, but it is only a fake plausibility. For one thing, the definer has an unholy advantage in giving arbitrary meaning to his subject-matter at the outset. Particularly in a discussion such as this, where terms are controversial and supposedly "subjective," the danger of "the fallacy of initial predication" is enormous. The very definition of value or of scientific method can decide automatically the issue between them (one way or the other) without necessity for subsequent argument. Another evidence of misleading plausibility is found in the realization that definitions illustrate but do not institute a point of view. They are ordinarily after-the-fact rationalizations of a pattern of thinking. For example, even when values are "defined" as indefinable or as primitive and unanalyzable, this, too, is indicative of a whole school of logic, itself completely sophisticated and unprimitive. Yet, these qualifications being noted, it still seems necessary to indicate, first, what one has in mind by the term "value."

Values are being looked upon here as second-order phenomena, not as immediate. That is, values are the outcomes of human choices, of choices among competing human interests. (The word "interest" is a very wide one, standing for all the dynamic human urges whatever they be called—impulse, drive, instinct, appetite, and so on. Nor does "interest" discriminate between native or conditioned factors.) If there is anything "immediate," it would be the interests themselves—but "immediate" does *not* signify being impervious to historical and genetic analysis, at least. All values, thus, are interests, but not all interests are values. Some "judgment"—not necessarily rational or even conscious—needs to be made before any interest in competition with another interest emerges as a value: a process of evaluation is required. As a most elementary illustration, the taste for, say, x becomes a value after it has been chosen over taste y. The taste x by itself is a candidate for the class of values; it is not automatically a member of the class. In ethics, the choices known as values are characteristically long-time preferences, involving those human interests regarded as unusually precious and intimate, yet capable, in general, of public expression and appreciation. But this does not seem to be an essential part of the definition of value. Nor does the question whether the choices are "good" or "bad." This is a third-order phenomenon of spectacular significance, but it does not introduce an essentially new element into the process of interest-plus-choice. It steps up that process into one of choice between values but it does not break the circuit.

No one needs to be told that this kind of definition, if not arbitrary, is in any event a loaded one and aimed in a certain direction.[1] What it is

[1] This is not necessarily an apology nor a confession, for a recent symposium, which devotes almost five hundred pages to "defining" and discussing values, winds up just about where it begins, *i.e.*, with the preliminary (and diverging) points of views of the contrasting writers simply re-enforced and defended. (See VALUE: A CO-OPERATIVE INQUIRY, edited by Ray Lepley, New York, Columbia University Press, 1949.)

designed to point up is the contrast between an approach which regards values as unique, unmodifiable bias-facts, immune to ordinary intelligent investigation, and one which sees them as amenable to "outside" factors such as those handled by psychology, anthropology, and cultural history. In more technical language, the first attitude believes the discussion of values in terms of "outside" factors to be an illustration of the naturalistic fallacy, which is the illicit attempt to make values derivative and "reducible" to some other dimension. The second attitude is inclined to view the alleged fallacy as a kind of myth, traceable, in part, to man's ceremonial reverence for certain words (like "ought," "moral," "duty," *et al.*) and to the institutionalized and invidious split between philosophy and science. The definition of value in the preceding paragraph is, of course, a frank expression of the second attitude; and such expression seems to be a prime function of definition. Before this definition can be employed, what about the term "scientific method?"

To introduce this phrase must prove embarrassing, since we all know what scientific method is! Almost anything that can be said will be accepted—and rejected. Certainly in the broadest sense, scientific inquiry is not cut off from the common sense activities of man in the solving of problems. The familiar five-step analysis by Dewey of an act of reflective thinking makes clear the easy transition from lay intelligence to scientific. In making that transition, however, different items can be emphasized, the emphasis itself disclosing pretty clearly an implicit philosophy of science. For example, the problem situation itself can be underscored and a considerable share of attention devoted to increasing the sensitivity to unsuspected deformations and challenges in man's environment. Again, inadequacies in the formulation of problems may be seen as focal, the result being an increased interest in sharpening the tools of diagnosis. Or, predictability can be made the core of method around which the entire scientific enterprise can be built. Here the stress can be on any one of the three factors of exact science as outlined, for example, by Margeneau: postulation of general principles, deduction of theorems, empirical verification. In other words, the "if-then" character of science can be made crucial. This, in turn, may mean concentration on technical experimental operations or it may mean the celebration of the attitude and behavior of the scientific man.

This is not a contention that there is anything necessarily divisive in these different interpretations. They complement one another and are all aspects of the general procedure by which man increases his grasp of any given set of phenomena. To emphasize any single aspect as *the* definition of science is important chiefly as an illustration of a point of view and a philosophy. This would be the case even if the broadest features of science were singled out, features like attitude of mind, on the one hand, or technological operations on the other. Elevation of the one cannot depress the other.

These two large interpretations of scientific method need to be carried a little further. The first interpretation—science as a temper of approach, a general way of handling problems—is, of course, extremely congenial to a philosophy that seeks to extend the orbit of science and to free it from the dominance of any solitary technique. Here scientific method is more than the routine of a laboratory, however essential that routine may be. Nothing less than a whole complex of human behavior and adjustment is what is indicated. This complex would include many elements. Perhaps the most significant would be the hypothetical spirit. The feeling for tentativeness and caution and the respect for probable error may indeed be the unique contribution scientific method can offer to human culture; it certainly is the only prophylactic against the authoritarian mystique so symptomatic of modern nerve failure. Scientists as men can, of course, resist the prophylaxis: we know of dogmatic and totalitarian "scientists." But they achieve that status only by sacrificing what, in their calling, is precious and irreplaceable.

Another obvious ingredient of the scientific spirit is experimentalism, the willingness to expose ideas to procedures and operations. When these are absent, ideas become meaningless and hypotheses barren. But operationalism is not simply a kind of gadgetry applicable only to physical or laboratory routine; it connotes a general attitude or methodological standard, to the effect that hypotheses of any nature in any field—even that of value—must carry with them the means for their clarification as well as for their justification or rejection. The experimental temper is a prime corrective of an imagination that otherwise might decline to what could be regarded as "merely speculative or philosophical." Equally corrective is the spirit of criticism. This, rather than the more familiar "objectivity," would seem a constituent of the scientific approach. For to be critical means more than to take nothing for granted, more than to be sceptical and impatient to be shown. It is not a synonym for indifference or neutrality. To be critical means to make decisions, to choose between alternatives, to say yes here and no there. And to choose signifies to establish a set of values. Scientific objectivity cannot be amoral in the sense of not giving a damn. Which does not mean that, *per contra,* the scientist is expected to load his dice or prefer alkalis to acids because he doesn't want his litmus paper to turn red. Caricatures like these cannot distort the functional connection between criticism and selectivity; even the pure physicist knows what he is after and what data are relevant before he enters his study or laboratory. Critical objectivity interpreted as the discounting of personal prejudice is elementary and unexceptionable: to take it as given should not, however, reduce it to triviality. And objectivity becomes trivial when it becomes neuter.

Attitude of mind, as was suggested above, is one of two broad descriptions of scientific method. The other is technological. What needs to be noticed here is that scientific operations are not all alike, nor are they the monopoly of physics and chemistry. Every field of experience demands

its own peculiar tools. There may be a common pool of instruments, such as those of measurement and counting, but no science needs to be "reduced" to inappropriate mathematical operations nor does it have to become "physicalistic" to become respectable. The travesty of insisting, because men and nations cannot be crammed into test-tubes or be stained and cross-sectioned for the microscope, that, therefore, the sciences of psychology or sociology are misnomers is an evasion of this whole point. Operations can be as unlike those of physics as carrying on a fight against delinquency through the use of playgrounds and club-houses, or changing a nation's entertainment habits from those of listening to those of seeing, or even working on a political committee during a crucial election. What *is* generally negotiable in science-as-technique is the operational spirit mentioned in the preceding paragraph. Non-physical experiments may be incredibly long-range; they may seem, at times, like desperate fumbling and uninspired improvisation. But when hypotheses are referred to operations, remote as they may seem to the physicist, the way is open for the self-correcting devices of science to exert their enormous power. The "if-then" form of scientific statement becomes now more than a bow to tentativeness or a tolerant testimony to sweetness and light: it is a prosperous sign that the spirit and technique of science are being significantly fused.

Is there anything inherent in science—either as a way of approach or as a set of operations—which necessarily eliminates it from any given area of experience? Is it on *a priori* grounds, on *theoretical* grounds, that scientific method has for so long been almost automatically excluded from fields such as ethics and values? Or is the exclusion cultural and institutional, an item of history rather than one of logic? These, of course, are largely rhetorical questions, for the above descriptions of values and of scientific method have already indicated the direction the present answers would take. But that direction may be made a little more explicit.

The last question refers back to an opening contention that the whole problem of the relation of science to values involves a cultural dualism which has become almost a hallmark of Western civilization (not that it is absent from other civilizations). To account for the character of Greek metaphysics or of early Christian theology and for their dominating roles in the ideological drama of the Western world is hardly on the present agenda. But what must be noted is that perhaps the prime concept contributed by ancient thought to modern is the schizoid notion of a metaphysical and epistemological split between appearance and reality. There has been an almost linear development of the idea of an absolute and precious realm of essences, on the one hand, immaculate and unchangeable; and, on the other, of an aberrant dimension of private sensation, subjective, relative, and untrustworthy. Philosophy deals honorifically with one, science vulgarly with the other. Ideals are in the first, human failings in the second. Social institutions themselves are judged sacred or secular as they fit themselves into these amazingly neat and contrived categories, categories which

have hypnotized even great scientists such as Galileo and Newton as much as they have the layman worrying about his body and his soul, this world and that, his hopes and his practices.

The compensatory character of this kind of dualism is so apt that it is startling. Genuine reality, the real McCoy, is the place where man's wishes are fulfilled, where frustrations are resolved, the happy hunting-ground. The world of *mere* appearance remains then as testimony to his failures; here, the animal did not always fall in the hunt, the thunder could not be controlled, the rains came and so did drought, and famine and disease and death. *Here,* the fumbling hand of experience unfortunately had to be relied upon. *There,* myth, magic, and metaphysics (if Dewey's analysis is correct) allowed man to escape a suffocating feeling of helplessness. The very word "reality" becomes a symbol not for existence but for value, and should really be followed by "loud cheers." This is an admittedly brusque statement of what has been recited at length many times. But it cannot be recited too often. The contemporary resurrection of absolutes in almost every field should demonstrate that.

What we are saying is that the dualism between philosophy (values) and science (facts) has been bequeathed to us, just as has the dichotomy between reality and appearances. It is not some hereditary dislocation. After all, it must not be forgotten that at one time the ambit of science did not include the stars, or disease and insanity, or even the drifting of the clouds or the working of a pump. These were precious and impervious. So, latterly, are human values. Institutionalized resistance to the Scientific Revolution could contract but it could not easily disappear.

But is the resistance only traditional? Is there not an intrinsic opaqueness to human decisions about taste, right and wrong, or even about the price market which must forever resist scientific handling? Consider, however, the suggestions already made about both values and scientific method. If values indeed derive from human choices and if scientific method relates to certain attitudes of approach plus a general commitment to instrumentalism of an "if-then" character, the gap between the two does not appear unbridgeable. Surely, objective, critical, and operational reports can be made about human decisions, and those reports, in turn, can themselves enter into the fabric of future decisions. The act of choosing can become a matter for investigation. So can the degree of approval or disapproval choices provoke, as well as the variety of choices and the possibility of discovering common elements between them. These empirical phenomena are not simply matters of casual introspection; they furnish the ground for much of social psychology, anthropology, and history. Nor is there anything which *automatically* prohibits human choices from accommodating themselves to tentativeness and provisionalism or even to the use of probable error. The compulsion which has deflected values from the realm of hypothesis to that of the absolute and unconditioned (and so to an area outside the province of intelligent inquiry) is but mere symptom of a compensatory hankering for prescientific certainties. And it would seem to be conditioned

rather than native. Values do not oscillate between the extremes of absolutism and of bare emotional improvisation because of some inner organic magnetism but (at least so it is being argued here) because of a cultural dualism which is itself amenable to historical and scientific analysis.

All this, it may be said, misses the real question at issue. For these allegedly "scientific" manipulations are only descriptive. They never reach the nerve of the problem. Reports and analyses of choices are irrelevant to normative decisions. They cannot tell us how to choose between our choices. What are good and bad choices? What *ought* we choose?

These august terms are expected to have a paralyzing effect and they are almost deliberately designed to arrest discussion. Yet these are words only and should be capable of a linguistic analysis which might remove some of their inhibiting influence. George Lundberg is but one of many who have made fruitful suggestions along this line.[2] He feels, for example, that the word "ought" is a predictive term and reducible to the familiar "if-then" form of any hypothetical statement. "We ought to avoid another war" is an elliptical phrasing of the proposition (or, rather, the propositional function) "if we want to avoid all the undesirable consequences involved in another war (*and we do*), then . . ." No element here, continues Professor Lundberg, including the usually suppressed "and we do," is exempt from the routine hypothesis-verification techniques of scientific method.

I believe an appeal like this is sound and I am convinced that semantic and symbolic analysis is vital as a necessary and preparatory step in relating values to science. But, as was suggested earlier in this paper, such analysis, even if necessary, is hardly sufficient either to convince the anti-naturalist or to overcome a culture barrier as high as that now separating fact from value. What more is needed to insure sufficiency is not to be found in an essay, or a book, or in the work of any single philosopher or scientist, even one as dominating as John Dewey. It is to be found (if it ever is to be found) in the co-operative activities of research workers and creative thinkers in all the areas where the very data for investigation are human choices. What is needed is the cross-fertilization already beginning to be employed in the most seminal branches of social science. The expanding new sciences of man are even now calling for the wholesale reconstruction of basic concepts: among those concepts are those of "fact" and "value." [3]

Such a direction, however, is not being indicated simply as an easy way of avoiding some of the responsibilities imposed by the present symposium. Even though it is sincerely believed that no exercise in logic or in polemics can replace the dogged, long-time energies required to bring values and sci-

[2] See his "Semantics and the Value Problem" in *Social Forces*, 27: 114–117, October, 1948. For a similar approach note, among others, the earlier work of Ray Lepley, *Verifiability of Value*, New York, Columbia University Press, 944; and the present writer's *Philosophy and the Social Order*, Boston, Houghton Mifflin, 1947; chap. 6.

[3] The readers of this magazine are, of course, familiar with the work referred to. Typical examples would be: the Hanover Institute experiments and those of Cantril at Princeton on perception; the group dynamics research at the University of Michigan; Kluckhohn's studies in cultural anthropology at Harvard; Sherman's and Mooney's "flash technique" investigations at Ohio State; and many others.

entific method together, some suggestions about value (bald and thin as they must be at this time) should be forthcoming; otherwise the point of the present controversy has not been faced. That point, to repeat it, is whether there is a way of choosing among values, whether there is some electric "good" which we "ought" to acknowledge. The following items are almost impertinently brief (and they have appeared before),[4] but perhaps they may stimulate reaction.

The locus for any evaluation of human choices has been clearly indicated in the foregoing pages: it is the process of free inquiry itself, the continuum of scientific decision. Such an emphasis assumes, of course, the functioning of other continuities, *e.g.*, the biological and the social. That is, concentration on the places of intelligence is a refinement of, not a substitute for, the techniques of survival. This recognition would constitute one of a cluster of assumptions or postulates, a cluster all the parts of which are intimately related. (The laying down of assumptions may be the quickest way of presenting a position.) Basic in such a set would be the possibly primitive notion that the resolution of conflict, the solving of problems, is "good." Perhaps equally primitive would be the assumption that a social group or individual organism be judged by its ability to grow as well as to survive. Less foundational would be the proposition that growth is determined by the degree to which men understand their surroundings—surroundings (or environment) *being interpreted in the broadest possible sense as a culture matrix involving biological and psychic facts as well as social and physical.* Understanding or knowledge (to continue) is a matter of doing, of control and change, not simply of contemplation. Conversely, among man's potent tools are his conceptual tools. To this can be added: the scientific enterprise is the symbol of this line of technological growth. (Terms like "technology" and "continuum" are used, despite some possible misinterpretation, because the first calls attention to the significance of tools and technics—conceptual as well as motor—in man's expanding comprehension and control of his surroundings; while the second notes the steady cultural development, even "progress," to be found in the history of human tools and therefore—of human control. There is a cumulative and Promethean power here which can neither be ignored nor put outside the dimension of human choice and its determiners.)

Here, then, is a naked list. It may suggest a clue to an "intrinsic" standard of value—"intrinsic" signifying that the standard is part of a culture and not imposed from without. It may also suggest that scientific method is not "merely" a means to some extramural end, but that it contains within itself a dynamic and self-correcting purpose for which no ethical apologies need be made.

There is nothing facile in any attempted reconciliation of values and

[4] Cf. *Value: A Co-operative Inquiry* (op. cit.), pp. 102 ff. See also the "technological" theory of value of C. E. Ayres found in his *Theory of Economic Progress,* Chapel Hill, University of North Carolina Press, 1944, Chaps. IV and X.

science. This is so obvious that it really needs no saying, but, for the record, it should be said so as to indicate that a possible charge of glibness has not been overlooked. Generations, if not centuries, of exploration in the new sciences of man may be required for a dualism so deeply rooted in Western Culture to yield. It may never yield and the race between our present versions of technology and morals may end in any one of the several catastrophic finishes we are asked to foresee almost very day. Not the facility but the necessity for engineering a change is what must be made clear. For what are the alternatives? What are the substitutes for scientific inquiry in the handling of human values? These questions must point up whatever may be important in these pages.

And they are not simply rhetorical questions. The answers can be found almost anywhere: in brute power or in mystical illumination, in retreat to The Church (whichever one) or in esoteric obscurantism, in Moscow or Rome, in Nirvana or on the Left Bank—the choices are generous ones. Are men driven to these—to absolutes, anarchy, or escape—because something called science has been tried and has failed. If there is any grosser libel than this it would be hard to find. Knowledge about man is barely beginning to be won; new disciplines are less than a generation old; methods of procedure are only now being devised. To propose that "Science" has had its chance—and look at the result! is, when honest, intellectual surrender of the most indefensible kind. The contrary is the case: the very methods of intelligent inquiry have been excluded from prosperous contact with whole segments of human experience, those of human decision, excluded not because of demonstrated failure but because of intstitutional blocks. Behind those blocks flourish now what have always flourished there—despair and absolutism. It is logically risky to employ strong disjunctions, but that values belong to science or to non-science seems evident. The crucial choice would seem to lie here.

THE VALUES OF SOCIAL SCIENTISTS

C. E. AYRES

AS PROFESSOR GEIGER knows, I am in substantially complete agreement with his analysis of the value problem. But I suspect that we are in the minority, perhaps a small minority. This may be because we are in the wrong, or it may be for other reasons. Since I find nothing in his paper at which to cavil except its brevity (and that is beyond his control), I propose to address myself (still more briefly) to the consideration of various possible reasons for the present division of opinion among the students of this problem.

Professor Geiger has suggested that the principal reason for the confusion that so persistently dogs our thinking about values is the immemorial dualism of appearance and reality. He regards this dualism as an error from which Western civilization is now in process of freeing itself; and I agree—both of us being greatly indebted to John Dewey at this crucial point. This seems to suggest that the whole trouble arises from the disposition of many of our colleagues to cling to a metaphysical tradition which he and I regard as obsolete. I do not mean to say that Professor Geiger intends to convey any such suggestion; for, as I shall try to indicate shortly, it seems to me to fall considerably short of the whole truth. Nevertheless, the suggestion is there. That inference will be drawn, I am quite sure, by many of our colleagues. Moreover, it will be resented; and the resentment so engendered will not tend to reduce our intellectual differences.

Speaking for myself alone, I wish to say at once that I think there is a modicum of truth in the suggestion I have mentioned—but only a modicum. The dualism of appearance and reality is an error; but it is not merely an error. As Professor Geiger indicates, this intellectual error is a derivative of the "cultural dualism which has become almost a hallmark of Western civilization." That cultural dualism is a fact, or perhaps I should say a condition, to be reckoned with in any definitive analysis of the value problem. It is a fact that two sorts of values will be found to prevail in every community, very definitely including our own. Moreover, the importance of this fact is impossible to exaggerate. The great danger is that we may fail to give it its full due. Indeed, it is just here, I think, that we "instrumentalists" (or whatever we are to be called) may have done most to deepen the confusion. At all events, this is a sin of omission of which I have lately come to realize that I have long been guilty.

I suspect that many of our contrary-minded colleagues understand people like Professor Geiger and myself (and even, perhaps, John Dewey) to be asserting that none but scientific values exist. If so, they are of course perfectly right in contradicting us. It is perfectly true that every culture includes a considerable admixture of irrationality—that values exist and are cherished with the highest degree of intensity which owe

virtually nothing to science but are virtually pure expressions of community sentiments and attitudes fortified by tribal legends and ceremonials of every kind.

To a very considerable extent, I believe, our professional opposition is that of this character. The discovery (or perhaps I should say recognition) of this aspect of culture is one of the most significant discoveries of the past century. That is why, for all its errors, *The Golden Bough* is a great classic. More than any other one book—so I would judge—it signalizes the recognition of the suffusion of all cultures by the irrational, the ceremonial, the extra-scientific. In large measure, I suspect, the insistence of the majority of present-day social scientists that values lie beyond the purview of science is intended by them to be an assertion that such values do in fact exist, and that they are by their very character extra-scientific.

Insofar as this is what they mean to assert, they are right beyond all question. Indeed, I think that we should try to make it clear that just this proposition is a major premise of our analysis. We do not deny the existence of irrational sentiments and convictions, nor the importance of such matters in the lives of all communities. We do deny that human life and culture consists solely of that sort of thing. But does anybody assert that? Not that I know of! All students of society recognize "the material culture" as a fact. All recognize the continuity of tool-using throughout the ages, and its generic identity with the instrument-making and using of modern science. Unless they did, it would be meaningless for them to assert that "values" are "extra-scientific."

To be sure, that assertion does mean somehing more. It means that the concept "value" is identified with the irrational and the ceremonial by definition. There is a difficulty here. But it is one that may be, at least in part, semantic. Whatever may be said of "values," no one can (and so far as I know, no one does) deny that choices and decisions of a sort are made in the context of science. Indeed, no one denies that knowing (in the scientific sense) affects choosing and deciding in all the affairs of life.

Granted there is no science from which one can learn which of two girls to marry, or which of two professions to follow. I suspect that some of the difficulty arises from this homely fact, or rather from a simple procedural error with respect to it. There is no science that tells us where to locate a bridge or how far into the future to predict eclipses. Mathematics has no answer to the question to how many decimal points simple division should be carried. Yet nobody treats such instances as evidence of the limitation of science. The point is that when we are thinking about science and its incidence upon the whole of human life we begin with science and proceed to trace its impacts wherever they occur. But when we are thinking about "values," we are likely to begin with some concrete personal situation with regard to which it is of course immediately evident that no one science is exclusively applicable or sufficient.

The significance of science for human life derives from the fact that

all experience is inter-related. Indeed, science may be defined as systematic, organized inquiry into the inter-relations of human experience. To say of any particular personal experience that it lies beyond the confines of science is as much as to say that here is a matter with regard to which an intelligent and informed judgment is impossible—a matter which can only be "decided" in blind and ignorant response to irrational sentiments and superstitions.

Curiously enough, nobody quite says that, not even the people who are most insistent in their (scientific) pronouncement that "values" lie beyond the limits of science. Nobody says it even of personal adjustments to community sentiments and mores. I suspect that sociologists and anthropologists often have occasion to advise the people they are studying with regard to their personal adjustments to sentiments which these social scientists do not feel and mores by which they themselves are not bound. I suspect them of saying on such occasions something like this: "The sensible and informed thing to do is to recognize community mores and sentiments as facts; to know what the community expects, and most especially those members of the community with whom one's life is inextricably interwoven; and to act with due regard, as an intelligent and informed person, to the probable behavior of other people."

But this is as much as to say (1) that experience consists of a series (or continuum) of relationships all of which are knowable and understandable; and (2) that knowing and understanding as much as one can is better than not doing so.

That is precisely what Professor Geiger, and the "instrumentalists" generally, are saying, and that's all they are saying. Such being the case, what is all the shouting about? Is it only a matter of words—of what we call things?

In a sense, it is; and yet the stubborn insistence of the majority of present-day social scientists that the word "values" can be properly applied *only* to matters of community sentiments and mores, and not to known and understood interconnectedness, is more than a mere terminological obsession. It is an obsession with sentiments and mores.

This obsession is itself a paradox; and as such I should like to refer it to my social-science colleagues for observations and analysis: the strange behavior of the twentieth-century social scientists! Very generally, it seems to me, we have succumbed to the lure of mystery. In certain respects human behavior is indeed mysterious. Here we have a species which has prospered and multiplied far beyond any of its fellow anthropoids, even far beyond any of its fellow mammalians. It has done so, very obviously, because of the tools and devices, including the master device of verbalization, which it has somehow managed to contrive. But all the while it has carried on in the mysterious fashion so graphically and encyclopedically described by Sir James Frazer—mysterious, because (as *we* know) it is all make-believe. There is nothing mysterious about tools, since they are quite obviously useful. It is the sheer uselessness and irrationality of the

behavior it describes that makes *The Golden Bough* a fascinating book.

But does this mean that myth, magic, and ceremonialism generally must be accepted at their own valuation as the real, in relation to which the world of tools is mere appearance, the end to which all else is but the means? That is what our colleagues seem to have concluded—quite overlooking the significance of their own achievements. For what distinguishes our age is not its make-believe but its knowledge, including the knowledge exemplified by Frazer.

What present-day social scientists most need, as Dewey has often remarked in similar situations, it a little more conceit. What they ought to be prepared to say is that knowledge of community sentiments and mores is more significant than mere blind experience of those phenomena. If they would do so, the value problem would be on the way to the archives of human achievement.

And sooner or later they will—not because of what Professor Geiger or any of the rest of us have said, but because of the inexorable pressure of facts. We are better off by virtue of such insight as we have gained into the social behavior of mankind; and that is as much as to say that being "better off" is indeed a matter of information and understanding, in short, science.

THE SCIENTIFIC ETHICS OF "BEYOND"

RAYMOND B. CATTELL

THERE are two ways in which science can have dealings with the problem of moral goals. It can study and classify, with complete objectivity and detachment, the religious systems of different places and times, in the spirit of the anthropologist and social psychologist, or it can attempt actually to build up a science of ethics, bringing scientific reasons for one course of action being better than another. The first is the daily concern of existing specialists and needs no comment. Our concern is with the second, which is automatically challenged by theologians, philosophers and some humanists who say, "The problem of values is forever beyond science."

These objections should not be taken very seriously, for it is easy to see that they spring in the main from thoughtless repetition of traditional views or from the professional territorialism which persists in defending the frontiers of obsolete intellectual kingdoms. Professor Geiger has courageously driven a clear highway through the debris of these defensive arguments and, being in agreement with his position, I propose to ask the reader to follow from the point to which such writers as he, Lundberg [11] and Lepley [10] have brought the question.

However, before proceeding, I should perhaps pause to point out that there is also a third, unfortunate and widespread commerce of science with religion, my reactions to which in recent articles [4, 5] may perhaps need to be "placed" in the reader's mind in relation to the present contribution. I refer to a bastard philosophy, very prevalent in some social sciences under the name of "action research," which mingles "revealed" religious ethics, i.e., non-scientific, ready-made ethics, with the experimental principles of social science and attempts to call the offspring "scientific" and to claim for it the prestige and character of science.

Every recommendation in "action research" argues first from a chain of truly scientific mechanisms—if A is done, then B and ultimately Z will follow—and secondly from a value system—because Z is good, A is good. It is my conviction that confusion of thought and damage to scientific prestige will result if we permit these callow enthusiasts to apply such labels as "research" or "science" to their recommendations unless they first demonstrate that their statement, "Z is good," can also be made from a scientific basis. This has not been done; indeed, naive "high school" moral values have been combined with "college" science without the glimmer of a realization that this great new possibility of a totally scientific ethics exists or that action research is 50% unscientific.

Of course, the second thread in the argument—the tracing of value sequences—can be legitimately based ultimately on non-scientific, revealed religion, providing this is done explicitly with a clear definition of what

Christianity or democracy or communism means to the writer, but at best it is a new wine in old bottles. The aim in social research must be to make ethics not only explicit but also scientific and empirical. The problem of scientific ethics—which is to be contrasted with the "revealed," intuitive, dogmatic ethics of the great religions—is to discover, define and defend some ultimate goal of "goodness"; for granted an ultimate good, the purely scientific principles of sociology and social psychology will eventually permit us to state what detailed actions and practices contribute to that goal and thus quantitatively to evalute the good and evil of various social and educational acts.

The first and classical attempt to put ethics on such a rational basis—that of Bentham and Mill in Utilitarianism [18]—could scarcely have been expected to succeed straightaway, but it pointed clearly to an objective, and unfortunately one which lesser social scientists since have neglected to attack. The closing pages of Dr. Geiger's essay call us anew to face this problem and tentatively suggest some carefully thought out goals such as the "degree to which men understand their surroundings" and the organism's "capacity to grow as well as to survive."

The hypothesis as to the unkown goal which in my opinion deserves most careful investigation and scientific debate I have already tentatively put forward in 1933 in the theory of "Evolutionary Ethics" [2] and integrated into the fuller context of the dynamics of religion in 1938.[3] It begins by substituting "progress" for Mill's "greatest happiness of the greatest number" and defines progress as the increase of the community's capacity to survive. This implies an increase in knowledge and control of the environment (including in environment the "biological and psychic facts" about the community itself, which Dr. Geiger rightly stresses, to avoid the common understanding which would narrow this knowledge to the non-spiritual knowledge of physical science).

In this initial glance at the implications of what I then labelled Evolutionary Ethics we will not immediately pursue the exact definition of this ultimate goal, which requires involved reasoning, and will pause only to point out that one does not use the term progress except as a general, popular designation of what is meant by this goal. For every educated person realizes that progress is a question-begging term. Suffice it for the present that our definition implies increase in knowledge and intelligence, as tested by the community's capacity to service in changing circumstanes.

Now if the greatest good is that which contributes to the birth processes of evolution in that great living stream of which man is part, we shall obtain our main orientation on ethics by a proper understanding of what the biologist, the geneticist and the social historian have discovered about the mechanisms of evolution. And here we encounter an astonishing fact. Although a magnificent vista of scientific understanding has been opened up by the geneticists, from Morgan to Muller, and *some* glimpses of the social process have emerged from such social scientists as Freud, Unwin and Toynbee, the ethical implications have been still-born into a

conspiracy of silence and have stood practically where they were adumbrated in Darwin's original writings! I say "adumbrated" because Darwin himself, despite a fresh and comprehensive mind which could not but set out on the right track, abandoned it before it led to any clear conclusion, partly because he was properly concerned at that time with his main thesis and partly, one suspects, because his own deep-rooted traditional religious beliefs presented an emotional barrier.

The chief feature of evolution—other than the standard sequence of variation, natural selection, and differential survival, which the student of ethics has to note, is that human evolution (and that of many mammals) has occurred largely through *competition among groups* rather than among individuals. The rejected ethics of "nature red in tooth and claw," which was conceived to be the meaning of Darwinism not only by the popular mind and in the rash emotionalism of Nietzsche but by Tennyson and the thoughtful world he represented, was an illusory perception which a little more thorough reading of Darwin would have eliminated. Thus Darwin pointed out [7] that when differential survival is applied to groups rather than to individuals "the tribe including the greater number of courageous, sympathetic and faithful members would succeed better." In short the qualities of altruism and selfless idealism which make the best community possible are advanced only where competition and selection occurs *among groups*. Not only is competition for survival the origin of altruism (rather than of the ruthless selfishness with which the thoughtless connect competition) but, as such biologists as Huxley have reminded us, competition frequently eliminates species and groups which specialize in hostility and heavy biological armaments. Success in competition is expressed more through economical, intelligent struggle with the physical environment than through warlike action against other groups.

Despite this common emphasis of traditional scientific ethics on self-sacrifice an apparent divergence still exists between the moral rules logically serving the goal of group survival and those which the great, universal, revealed religions — Buddhism, Christianity, Mohammedanism — have taught from the intuitions of their founders. This contrast has been most explicitly discussed in the present generation by McDougall [12] under the concepts of "nationalist" and "universalist" ethics. But the same contrast has been formulated from one viewpoint or another by the few outstanding thinkers since Darwin who have continued to keep contact with the problem. It is very clearly stated, for example, in Sir Arthur Keith's recent great panorama of evolution [9] where he points out that Herbert Spencer formulated the same distinction under the terms "the code of enmity" and "the cody of amity," [14] though these terms are more clever than apt.

Although the most original minority of social thinkers, as exemplified by Darwin, Kropotkin, Spencer, McDougall, Keith, Huxley, Hooton,[8] Lundberg,[11] Lepley,[10] and especially Allee,[1] has thus maintained and clarified a perception of the fact that there are naturally two codes of ethics and that one of them (the "national" code) is closely connected

with the evolution of altruistic social capacities and attitudes, there has been no attempt, except in the hypothesis of "cooperative competition" mentioned above [2] to reconcile their apparent divergence. Indeed, though a cautious thinker like Keith simply describes the two codes without a value judgment,[1] these writers have generally taken implicitly a Nietzschean view that the two codes are violently opposed and have varied between a veiled scorn of the illogicality and sentimentality of universalist principles, on the one hand, and the intellectually uncomfortable position of paying lip service to universalism while their logic leads to nationalist ethics on the other.

As I have attempted to show [2, 3, 6] the solution lies in recognizing that the competition of human groups is for the good of humanity as a whole. Our cultures and races are experimental variations in a common endeavor in which only a few will survive—just as the different mixtures in the chemist's row of test tubes are part of a single endeavor though only the best emergent reagent is eventually used. This subordinates nationalist ethics to universalist ethics in that it requires the maintenance of the rules of the game to be more important than the winning of the game—in the values of any competitor. But it also modifies universalist ethics—at least as they are sometimes taught—by maintaining that there are more ideals than the greatest peace and comfort of the greatest number. Indeed it may well turn out to be an historical social-psychological fact that the debased, over-simplified forms of universalist religions arose by a mistake of thinking—by an act of wishful thinking which extended the habits of thought engendered in nationalist religion *beyond* the boundary of the group.[11]

For clarity of discussion one must therefore recognize two forms of universalistic ethics, the first having the above mentioned crass aim of universal sameness, safety, and satisfaction of instinctual needs—as exemplified in Communism and some interpretations of Buddhism and Christianity—and the second recognizing the role of intercultural competition ("Render unto Caesar . . .") and emphasizing individuality in relation to some greater end than the tower of Babel, as in the more spiritual interpre-

[1] "My main thesis (is) that right down to the dawn of civilization the habitable earth formed a mosaic of separated territories and peoples, and that such a group favors rapid evolutionary change." (9, p. 3.)

[11] This is written in a frame of reference which supposes that social change has occurred through reward and punishment of a vast number of tentative, random, trial and error, blind, emotionally (or materially) determined innovations. Most of these social mutations will be attempts to escape from the reality principle and so, as with biological variations, the majority will be for the worse and only a minority will survive. Two thousand years' survival may be enough to prove that an ethic is sound, but we must remember that Christianity, at any rate, has fortunately not been interpreted, in its most vigorous period, as a simple extension of nationalist ethics to include all mankind in the in-group.("I come not to bring peace, but a sword.") At present there is a tendency to interpret it as universal indulgence, as a kind of world communism, but in essence it has always had more regard for the soul than the body, for the future than for the present. Thus, in its emphasis on struggle directed to a future purpose, its emotional values are very close to those reached on scientific grounds by evolutionary ethics.

tations of Christianity. The first might be called united hedonism while the second is an intuitive adjustment to that pluralistic search for the single goal of progress which becomes intelligible in the light of a scientific analysis of ethics. Usually, for example in McDougall's discussion of the problem, "universalistic" has meant uni-hedonic, for it is this which is a true opposite to nationalist ethics; whereas the Western interpretation of revealed Christianity and the present explicit scientific argument for evolutionary ethics transcend both of these extremes and reconcile their main features in a more subtle common goal. Evolutionary ethics leads in fact to what has been described in an earlier attempt [2] at clarification as "cooperative competition."

This concept of cooperative competition among groups, of honoring those we disagree with even while we oppose them, because, like us, they are testing out an idea for us all, by living it, resolves the chief moral uncertainties which torment modern man in practical life. It shows, for example, that those who want to end war by ending separate nationalities are throwing away the baby with the bath water. If we are to have world unity it must be only as a means to world partition. Since no one knows what genetic, racial changes or cultural innovations are in fact "progressive," the world must be divided into a great number of racial and cultural experiments. To parody Voltaire, "If races did not exist it would be necessary to invent them." The great advantage of a world federal government would be that these different experiments could be made more efficient experiments than they are today, by having a central clearing house in which records of the success of various social and genetic experiments could be more sensitively compared than is possible today and the information more rapidly distributed to guide all participants in the cooperative competition. These principles also indicate that indiscriminate migration is a mistake. The low browed Neanderthaler was replaced by modern man (as Keith points out [19]) because the latter had an opportunity to inbreed, in comparative isolation, away from the Neanderthal prevalent type. An experiment, both in cultural and biological aspects, may need a few thousand years of trial before it is thrown back into the mixture. Crude universalist ethics do not recognize these needs, assuming that because men are equal in potentiality they must be equal in function and indefinitely interchangeable.

With this brief review of what has been called "Evolutionary Ethics," [2] its origin as a scientifically derivable moral goal and its practical implications (in those small respects in which they differ from those we have blindly reached by intuitive religion and patriotism) we must return to the thorny issue we by-passed earlier. What is the criterion of whether a social experiment is a success, i. e., what is progress? The first proof that one set of social customs or one pattern of genes is more adaptive and successful than another is that the society carrying them lasts when others pass out of history. But this is not enough, for in a simple, fixed environment very primitive forms sometimes last as long as those we would con-

sider higher. The criterion then becomes "To what range of change could this organism or community of organisms adapt sufficiently to survive?"

So great a problem as this question presents cannot be handled respectfully in this space; but at least we can see that the criterion is one of being prepared to meet demands *beyond* those of the visible environment and of intelligently anticipating possible threats to survival. The concept of attention to what is "beyond" the given is present in another sense in the whole evolutionary process. The latter consists essentially in both its biological and social aspects, of what the psychologist calls "trial and error learning." Life adventures into new space in the universe and is punished until it finds the real shape of that space, i. e., until it gains adaptations which fit the ineluctable principles of that region of the universe in which it has attempted to establish itself. The more of the universe it occupies and "understands" the more it has achieved safety even in the smaller region it originally occupied. It is for this reason that in the fuller and more systematic treatment [6] of this science of morality I have labelled the whole development the *Ethics of Beyond,* as being a more essential designation than evolutionary ethics.

That this and other, more tentative movements toward developing ethics out of science have as yet received so little discussion and investigation is readily understandable in the light of social conditions. The overwhelming majority of those who are keenly interested in questions of morality and ethics are religious teachers wedded to revealed religions. To them a scientific approach is incomprehensive and anathema. The remaining and smaller group—the philosophers—cannot handle something that is too much alive and too much in earnest to be put in an intellectual museum. The development of evolutionary ethics or any other scientifically-based approach to discovering moral principles will presumably have to come from the scientists themselves—men who combine the intellectual objectivity of the philosopher with the burning purposefulness of living religion. When social science has more scientists in its ranks and comes of age it is certain that a great science of ethics will develop whereby the friction of communities, internal and external, will give way to a new beauty of creative functioning.

[1] Allee, W. C., *The Social Life of Animals,* New York, W. W. Norton Co., 1938.
[2] Cattell, R. B., *Psychology and Social Progress,* London, C. W. Daniel Co., 1933.
[3] Cattell, R. B., *Psychology and the Religious Quest,* New York, Nelson and Son, 1938.
[4] Cattell, R. B., "Ethics and the Social Sciences," *American Psychologist,* 3: 193–198, 1948.
[5] Cattell, R. B., "The Integration of Science with Human and Moral Values," *British Journal of Psychology,* (In Press), 1951.
[6] Cattell, R. B., *Beyond: The Science and Ethics of Progress.* (In Preparation.)
[7] Darwin, C., *The Descent of Man,* London, Murray, 1913, p. 199.
[8] Hooton, E. A., *Man's Poor Relations,* New York, Doubleday and Co., 1942.
[9] Keith, Sir J. A., *A New Theory of Human Evolution,* New York, Philosophical Library, 1949.
[10] Lepley, R., *Verifiability of Value,* New York, Columbia University Press, 1944.

[11] Lundberg, G. A., "Semantics and the Value Problem," *Social Forces*, 27: 114–117, 1948.
[12] McDougall, W., *Ethics and Some Modern World Problems*, London, Methuen, 1924.
[13] Mill, J. S., *Utilitarianism*, London, Parker, Son, and Bourn, 1863.
[14] Spencer, H., *Principles of Ethics*, London, Williams and Norgate, 1892.

VALUES AND SCIENCE

FRANZ ALEXANDER

WHILE I am in thorough agreement with Professor Geiger's position that the problem of "values" is both a legitimate and possible subject of scientific inquiry, the discussion of this topic appears to me as much outmoded as a controversy over whether machines heavier than air can rise up against the force of gravity and fly. Both of these questions are settled by the actual demonstration of their feasibility. That section of human behavior which pertains to values (evaluation, preference, choice) has been and is being successfully investigated by scientific methods of observation and reasoning. The dichotomy between "facts and values" is a pseudo-distinction and the problem of whether values belong to a realm which is beyond the reach of scientific methods is a pseudo-problem. This being my position, little remains for me to add to Dr. Geiger's scholarly treatise—little more than to refer the reader to the pertinent literature.

First, in order to delineate the problem, the meaning of the expression, "value" and "evaluation," should be defined. We may well agree upon Professor Geiger's proposition that "values are outcomes of human choice." Taste in the narrower and broader sense is the simplest example—ethical value systems the most complex. Every choice referred to by adjectives—good or bad, beautiful or ugly, useful or useless, worthwhile or unworthy—belongs to the phenomenology of values. The fallacious belief that such value judgment cannot be studied scientifically can be explained partly from such cultural-historical reasons as mentioned by Professor Geiger, partly from the fact that these predelictions are perceived as compelling forces which do not require further explanation or justification. Preferring blondes or sweets is not the result of reasoning, and "beautiful" is a judgment which is immediate and does not need the explanation of an aesthetician. The same is true for moral values such as good or bad. One may try to explain post factum why an act is felt good or bad, but no explanations are needed to make such decisions. In fact, when a logical deduction is needed for a choice, we do not deal with the same type of phenomenon as, for example, a move in playing chess. The latter type of choice easily lends itself to scientific explanations by deductive reasoning. The immediate subjective nature of value judgments is one of the reasons why they have been considered as being beyond rational analysis. According to the traditional postulate of philosophy, ethical and esthetic values have their own immanent laws which are inaccessible to the accepted method of science. The first part of this statement is valid: namely, that the phenomena of aesthetic beauty and morality have their own immanent laws, but the second part of the statement, that these laws are beyond the realm of science, is but the reflection of the state of our ignorance which prevailed at the time when this dogma was postulated It is noteworthy

that in the field of the more prosaic physical appetites, such as sexual or gustatory appeal, the dogma of their inscrutability has never been postulated. Although the laws governing such predilections are not known, it is generally assumed that the nature of the person or of the organism determines in some unknown manner what sexual partner or food will appeal. (For example: "Les extremes se touchent.") Those who defend the dichotomy between "values and facts" obviously must consider aesthetic and moral values as essentially different from the more primitive value judgments in the field of gustatory or sexual taste. The self-contradictory nature of this is easily demonstrated by the irrefutable argument that moral and aesthetic judgments take place in the human organism and therefore must follow the same basic principles which govern all other phenomena of life.

As stated above, however, all this logical quibbling is unnecessary and outdated today, since the phenomena of moral choice and aesthetic appeal, as well as of gustatory and sexual preferences, have been studied with considerable success by the methods of biology, psychology and social anthropology. The following are only a few references to the most important studies made in this field.

(1) *Gustatory values*—It has been demonstrated by Richter and others that in animals the choice of food is largely determined by the organism's physiological needs. Rats who were deprived experimentally of certain ingredients in their food chose those foodstuffs which contained the needed ingredients.[1,1a]

(2) *Sexual appeal*—Psychoanalytic studies have shown that sexual attractions (extreme cases of fetishism, for example) are largely determined by previous experiences (fixation). Furthermore, it is well established that sexual tastes are governed to a large extent by such phenomena as incest taboo and incestuous fixation. Incest taboo may account for the sexual appeal of persons whose physical or mental attitudes are different from the male or female member of the family, and, conversely, fixations on the family members of the opposite sex will favor the choice of partners who resemble them. These are only some of the determining factors; accidental experiences of early life also contribute to determining sexual appeal in later life.[2,3,4]

(3) *Aesthetic values*—In this almost virgin field of inquiry, the most fundamental studies are those of Freud on wit and humor. He accounted for the witty effect by a specific combination of form and content which allows gratification for repressed emotions. The pleasurable effect expressed in laughter is derived from the saving of energy which is liberated by lifting repressions.[5] Kris applied the same principle to explain the effect of caricatures.[6] Assuming that the tragic effect in drama and epos belongs to the same category of phenomena (aesthetic effect), Rank and Sachs explained this in terms of dynamic psychology. In addition to the effect of form (rhyme, meter, economy of style) the reader, through identification with the suffering hero, can gratify his forbidden longings.[7,8] The author

of this article attempted to apply the same principles (pleasure derived from saving psychological energy by the combined effect of form and content) to all varieties of aesthetic appeal.[9,10] Without claiming that the psychodynamics of aesthetic appeal is fully explored in all its details, these studies sufficiently demonstrate the accessibility of aesthetic appeal to exploration by the methods of psychology.

(4) *Moral Systems of Value*—The "functionalist school" of social anthropology explains various social institutions, habits and value systems as adaptations of the members of the group to the total social structure in which they live.[11,12] An example is the explanation of the Japanese emphasis on ancestor worship as an essential feature of the feudal system which persisted in Japan uninterruptedly over many centuries.[13] This specific form of morality is not only the reflection of the feudal ideology in the mind of the individual, but it is also an indispensable guarantee for the survival of the feudal system. Likewise the emphasis upon the self-made man and the depreciation of authority worship in the United States are expressions of the American social structure and American history. The social function of these attitudes in a competitive democratic civilization is obvious.[14,15,16]

The explanation of all social institutions and moral systems from their survival value has its limitations, however. First of all, moral value systems change more slowly than the social conditions which originated them. Consequently, many institutions and value systems lose their original function of perpetuating the survival of the social system. They become outmoded, adjusted to past conditions and unfitted to the present. (cultural lag)[17,18] Unless the history of a social group is known, the function of an institution or value system is often not apparent. Since social anthropologists most frequently study so-called primitive cultures which have no written history, the functional theory cannot always be applied successfully to the explanation of social institutions, habits and mores prevailing in such societies.

While these studies in the field of social anthropology have shed light upon the variations of moral value systems, the fundamental moral propensity of human nature is not explained by them.

Man's basic moral propensity can be understood in the light of psychoanalytic theory as the psychological manifestation of the process of biological maturation during which the person's capacity to replace love of himself with love for another gradually increases.[19] I have described this phenomenon as the manifestation of the biological "surplus principle." As long as the organism grows, intake and retention of matter and energy outweigh their expenditure. Otherwise growth would not be possible. Accordingly, the emotional orientation of the immature organism is primarily self-centered. The process of growth, however, has a natural limit: when a biological unit reaches a certain size, addition of substance and energy becomes impossible because its capacity to organize living matter in one system reaches its limit. Individual growth then stops, and surplus energy is released in the form of propagation. The psychological concomitant of this biological maturation is the replacement of self love by object love.

It manifests itself in generous outwardly directed creative attitudes of the healthy mature person which in ethics is called altruism. Without this capacity for interest in and love for others, no social organization can exist since every form of social organization requires from the individual a certain amount of consideration for others with a simultaneous curtailment of concentration on the promotion of self interest.[20,21]

Though moral systems may differ radically in detail, they have one feature in common: they all require a consideration of the interests of other members of the society with greater or less sacrifice of selfish interests. Merely on the basis of "you must," no society could survive. What makes it possible for the moral "must" to become a vital force is the biological and psychological phenomenon of excess energy which is not needed for self preservation and which can be expended for propagation and neighborly love. Only in this way can a group be successfully preserved.

A social organization in which respect for each other's interests is based alone on coercion, on fear of law or of a tyrannical leader, has never been proved stable. Internal frictions caused by competing interests eventually break up the cohesion of such a system. Social organizations can only be explained by the existence of a cohesive force which Freud called "eros" and which this author describes as the surplus energy of the mature organism—energy not needed for self-preservation and expendable for creative activity and object love. According to this view, altruism has a biological foundation; it appears in its most undiluted form in the maternal instinct. I assume that this productive and creative faculty is what Kluckhohn had in mind when he postulated in addition to the utilitarian (functional) principle in culture, the existence of another culture forming principle. (loc. cit.)

Such anthropological and psychodynamic considerations validate the contention that moral phenomena can be studied by the methods of science and retraced to universal biological and psychological principles. These phenomena can be explained partially as adaptive mechanisms and partially as the manifestations of biological and psychological maturation, which allows the organism to apply some of its energies not alone to its own growth and survival but also to the preservation and benefit of others.

[1] Richter, C. P., Holt, L. E., Jr., and Barelare, B., Jr., "Nutritional Requirements for Normal Growth and Reproduction in Rats Studied by the Self-Selection Method," *Amer. J. Physiol.* 122:734, 1938.

[1a] Richter, C. P., "Biology of Drives," *Psychosomatic Medicine,* 3:103, 1941.

[2] Freud, Sigmund, *Three Contributions to the Theory of Sex,* Fourth Edition, New York, Nervous and Mental Disease Publishing Co., 1930. (Contribution III: "The Transformations of Puberty." pp. 84–85)

[3] Freud, Sigmund, "Contributions to the Psychology of Love." "The Most Prevalent Form of Degradation in Erotic Life." (In his *Collected Papers,* Vol. 4, Paper No. 12).

[4] Alexander, Franz, *The Psychoanalysis of the Total Personality,* New York, Nervous and Mental Disease Publishing Co., 1935. (Lecture 7: "Instinct Fusions—A Case of Masochistic Transvestitism," pp. 132–138.)

[5] Freud, Sigmund, "Wit and Its Relation to the Unconscious," (In *The Basic Writings of Sigmund Freud,* New York, Modern Library, 1938, pp. 633–803.)

[6] Kris, Ernst, "The Psychology of Caricature," *International Journal of Psycho-Analysis,* 17:285, 1936.

[7] Rank, Otto and Sachs, Hahns, "The Significance of Psychoanalysis for the Mental Sciences," *Psychoanalytic Review*, 2:297–326, 428–457 (1915); 3:69–89, 189–214, 318–335, 1916.

[8] Sachs, Hanns, *Gemeinsame Tagtraume*, Leipzig, Internationaler Psychoanalytischer Verlag, 1924, pp. 1–36.

[9] Alexander, Franz, "Unconscious Factors in Aesthetic Appeal," (Deuxieme Congres International d'Esthetique et de Science de l'Art, Paris, 1937, Tome I. Paris, Librairie Felix Alcan, 1937, pp. 217–220.)

[10] Alexander, Franz, *Fundamentals of Psychoanalysis*, New York, W. W. Norton, 1948, (Chapter 8: "Unconscious Factors in Wit and Aesthetic Appeal.")

[11] Kluckhohn, Clyde, "The Limitations of Adaptation and Adjustment as Concepts for Understanding Cultural Behavior." (In Romano, John, *ed. Adaptation*, pp. 99–113.)

[12] Kardiner, Abram, *The Individual and His Society*, New York, Columbia University Press, 1946.

[13] Benedict, Ruth, *The Chrysanthemum and the Sword*, Boston, Houghton Mifflin Co., 1946.

[14] Mead, Margaret, *And Keep Your Powder Dry*, New York, Wm. Morrow and Co., 1942.

[15] Gorer, Geoffrey, *The American People*, New York, W. W. Norton & Co., 1948.

[16] Alexander, Franz, *Our Age of Unreason*, Philadelphia, J. P. Lippincott Co., 1942, pp. 294–312.

[17] Ogburn, W. F., *Social Change with Respect to Culture and Original Nature*, New York, B. W. Huebsch, 1923.

[18] Alexander, Franz, *Our Age of Unreason*, Philadelphia, J. P. Lippincott Co., 1942, pp. 131–138.

[19] Freud, Sigmund, "On Narcissism: An Introduction," (In his *Collected Papers*, Vol. 4, Paper No. 3).

[20] Alexander, Franz, *Fundamentals of Psychoanalysis*, New York, W. W. Norton 1948, pp. 41–48 and 75–81.

[21] Alexander, Franz, "Emotional Maturity," Illinois Society for Mental Hygiene, *Mental Health Bulletin*, Vol. 26, No. 5 Nov.-Dec., 1948.

SCIENCE IN SERVICE OF VALUES

HENRY N. WIEMAN

I AGREE with Mr. Geiger that scientific inquiry can discover, describe, appraise, predict and control values and promote their increase; that our culture today suffers lamentably because the sciences have not been devoted to this task as fully as they can be; that our failure to apply the sciences in this way is in part due to certain historical developments and institutions and not to the intrinsic nature of the problem. I would add that our failure is further due to the delinquency of philosophy in not having analyzed and interpreted value in such a way as to make it readily accessible to inquiry by the various sciences.

My program, then, shall be as follows: First, to set forth an analysis of value, hoping to show that it opens the way to scientific inquiry into this realm more adequately than Mr. Greiger's interest-theory can do; second, to look at alleged objections to applying science to the problems concerning values.

What is enjoyed and suffered is always a complex situation. It is never merely an object of interest such as, for example, a spoonful of ice cream in transit to the mouth. The complexity of the psycho-biological, social and historical individual is vastly greater than this interest in ice cream. Consequently the conditions that must be present in the situation to render it enjoyable to such an individual must include far more than contemplating or consuming ice cream. This disparity between the complexity of what is actually enjoyed or suffered—the total set of conditions necessary to the enjoyment—and the satisfaction of an interest can be easily demonstrated. Change the social conditions so that approval turns to contemptuous social disapproval as I consume the ice cream; or apply a red hot iron to my leg; or change my cultural conditioning. When any of these things happen, behavior reveals that eating ice cream yields no enjoyment whatever unless many other conditions are present. At best the ice cream is only one component, perhaps a minor component, in the total set of conditions that yield the enjoyment.

It is not infrequently the case that the object which satisfies the interest is not needed at all to make the situation enjoyable. The situation might even be more enjoyable if the object were obliterated. Depth psychology has demonstrated this beyond a doubt, I think. What the subject really enjoys may be very different from, even impaired by, the satisfying of the interest, so complex and many-layered is the social, psychological, historical and biological individual.

The term "historical" in the preceding sentence refers to the fact that the human individual accumulates the past and lives by it to a very high degree. Specifically it refers to situations enjoyed and suffered in the past of the individual and in the past of his culture, which in great part

determine the kind of situations he can enjoy. The term "social" refers to the fact that what any individual can enjoy is determined in no small part by what others enjoy and suffer who are associated with him.

The drive for food or shelter or marriage or free enterprise or world domination or the American Way of Life may be quite destructive of those conditions necessary to render situations enjoyable for the persons who are driven by these interests. The common answer to this assertion is to say that interests can be identified with values only after they have been criticized and corrected by knowledge of their conditions and the consequences of their satisfaction. But this does not meet the issue. All the interests of which we are conscious, hence all that we can possibly criticize and correct by such investigation, may be inimical to the conditions needed to make the situation enjoyable. Hence these interests must be undercut by inquiry into these conditions which lie outside the range of conscious interests until scientific search uncovers them. Only then can conscious interests be treated to criticism, re-education or whatever scientific research may reveal to be the best way to transform them to the end that situations may be enjoyable or more enjoyable. As a matter of fact, doctors and clinical psychologists do precisely this in many cases. So also do sociologists when they try to find out why a social group is relatively miserable or relatively happy.

If I understood Professor Geiger, he says that interest must be educated and disciplined until it follows faithfully the guidance of scientific inquiry and the pursuit of scientific knowledge. With this I agree, but it is not enough. Scientific inquiry, according to the argument here set forth, cannot lead to greater value unless it searches not merely the conditions and consequences of human interests, but also and more important it must uncover the conditions which lie beyond the scope of interests and often demand the radical transformation of interests before the situation can be enjoyable.

If science is to gain the knowledge required to conserve and increase values it must discover: (1) the conditions which render situations progressively enjoyable to the individual, recognizing that these conditions are beyond the reach of the daily interests of the individual and often in opposition to these interests; (2) the conditions which relate situations enjoyed by one individual to those enjoyed by others in such a way that these diverse situations sustain and enhance the enjoyableness found in each; (3) the conditions under which situations yielding enjoyment also at the time build meanings that link the present situation with past and future and with situations enjoyed by others. This last is especially important because it is under the guidance of such meanings connecting each enjoyable situation with many others, that action can be directed to conserve and increase values for all concerned.

An example of this kind of scientific research is set forth by Peter Drucker in a series of three articles appearing in Harpers Magazine for November and December, 1946, and January, 1947, under the title, "The Way to Industrial Peace." He draws especially upon the work of Elton

Mayo but also upon many others and upon his own research. This research is directed to the problem of how to organize and conduct industry so that the industrial worker will find his work-situation enjoyable and at the same time develop in his work those meanings which relate what he does to what others are doing so that the enjoyableness of the situation of each sustains and enhances the enjoyableness of the situation of others associated in the industrial undertaking. The conditions that must be set up for all this to occur cannot be found by following the lead of the interests of workers and management. It had to be done in opposition to these conscious interests and could be accomplished only by transforming them.[1] Much research in the field of personality and education points in the same direction.[2]

There is one process in particular which, more than any other, I believe, creates situations that are most enjoyable for human beings. The researches, previously noted, together with most clinical psychology and the psychology of personality, indicate such to be the case. The process can be analyzed into three component parts: (1) Getting the viewpoint of the other person in the sense of seeing and feeling the world as he does so far as possible, regardless of whether you think him mistaken or not, or approve his goals and drives. Indeed you cannot know if he is mistaken, nor have any defensible ground for disapproval, until this intersubjective apprehension occurs. (2) Integrating these views and feelings derived from the other with your own in the sense that they become a part of your personal equipment, guiding your conduct in dealing with that other person and with the world of humanity generally. Chiefly through this acquisition of what others think and feel does each individual expand the range of his appreciable world and enrich it with felt qualities more vivid and varied than any world could be for the individual without such acquisitions. (3) Expressing yourself under conditions which enable others to get your thoughts and feelings in like manner as you do theirs.

Analysis of the scientific researches given in the notes and above into the conduct of industry, the conduct of education, the conduct of the home and into the developing personality of the child, reveals, I think, that it is this threefold process which makes situations enjoyable, meaningful and mutually enhancing for all associated individuals.

Perhaps it can be said that the master need of a self-conscious human personality is to feel that he himself, what he is, thinks, feels, does, makes an important difference in the world of things and in the minds of others

---

[1] Similar scientific research with the same constructive outcome is described by R. W. Johnson in *Or Forfeit Freedom,* Garden City, N.Y., Doubleday, 1947. For an account of many other instances of such scientific research see the articles by Drucker above noted, also his recent book *The New Society,* New York, Harper and Bros., 1950, and other books by him; also works by Elton Mayo.

[2] Sherif, M., *An Outline of Social Psychology,* New York, Harper and Bros., 1948; Sherif, M., and Cantril, H., *The Psychology of Ego-involvements,* New York, J. Wiley and Sons, 1947; Dreikurs, R., *The Challenge of Parenthood,* New York, Duell, Sloan and Pearce, 1948; Murphy, Gardner, *Personality,* New York, Harper and Bros. 1947.

associated with him. But this master need can be satisfied for associated individuals only by the three-fold process above described. When conditions do not permit this three-fold process to occur effectively, the individual seeks compensatory satisfaction by way of the "authoritarian personality," the "destructive personality," the "retractive personality," and the rubber stamp or conformist personality.[3]

Community between individuals is achieved by the threefold process and can be widened and deepened only as conditions are provided enabling this process to operate more widely and fully. The human mind and personality are developed in the infant by this threefold process. Indeed without this process the infant could never learn to talk, because the use of language involves this intersubjective transfer of meanings. The individual can absorb the values of his culture or of any culture only by way of this threefold process. To the measure this process is allowed to operate by providing the required conditions, conflicts cease to be destructive and become constructive of human values. Freedom, in the sense of being spontaneous and creative, can be had to the measure conditions permit this process to occur between associated persons. Without it there can be no such freedom.

This process of internalizing the attitudes and feelings of others can become pathological or evil only when it assumes forms which prevent its own expansion to other persons and its progressive deepening. What is called objectivity in a novelist portraying the thoughts and feelings of people illustrates the right way for this process to occur. One must not identify himself with the other person nor become obsessed with his feelings. To do so would not be true to our description of the process, for in such case the process would stop with the first of the three parts and leave out the second and third. A surgeon cannot perform effectively if he identifies himself with the feelings of the patient, but a good surgeon can be aware of the feelings of his patient and act accordingly.

If this three-fold process is creative of all human values over and above those acquired by direct action between organism and physical environment (and even these are caught up and transformed by the three-fold process), then it should be the task and responsibility of all the sciences to search out in every walk of life those conditions most favorable for its occurrence. These conditions are physical, chemical, biological, psychological, social and historical.[4]

This analysis and interpretation of the problem of applying science to human values evades, I think, most of the arguments that have been raised against such an undertaking.

One of the arguments against the claim that science can and should be applied to values, is based upon the assumption that values are subjective while science can only deal with objective matters of fact. But when value

---

[3] This analysis is in part taken from Eric Fromm's *Escape from Freedom*, New York, Farrar & Rinehart, 1941. This work is an excellent example of the kind of scientific research for which I am pleading.

[4] The meaning of "historical" in this context was above stated.

is identified with the existential conditions and their possibilities which are enjoyed and suffered by historical, social, psychological and biological individuals, value is no more subjective than any other fact of nature.

Another argument against the application of science to value is that you cannot persuade a man that what he does not like is better for him than what he does like. Here again is an implicit identification of value with interests. In any case, the arts of persuasion and education are a part of this total problem. They are practical problems to be solved under the guidance of knowledge gained by scientific inquiry.

Another argument on the side of the opposition is that value refers to what ought to be, while science can describe only what is. Both these assumptions are wrong. Science does not merely describe what is; it often describes what never has occurred but will occur under specified conditions, probably and approximately. On the other hand, values identified with the complex set of conditions actually enjoyed are not merely what ought to be, but are a part of what is. On the other hand, an ideal which can never be actualized, say an ideal friendship which no one ever experiences as an actuality, does not have so much value as the actual friendships which men do experience. Ideals have their place as guides for conduct. But they do not have as much value as actual situations, except to the measure that they can be actualized, and then of course they have to that degree changed from ideals to actualities.

The sense of moral obligation is a very important fact, but it develops out of the experience of actual situations. The conditions that foster and develop it, that inform it with required knowledge, that correct it when it goes astray and become pernicious, as it sometimes does, all these conditions must be searched out by the revelant sciences, if moral obligation is to play its rightful part in human life.

There is nothing in the nature of a moral ideal which excludes it from scientific inquiry, criticism and correction. Of course, if moral ideals are the greatest or highest values, than nothing discovered by science or by anything else can be applied to their criticism or correction. But if all the values of human life, including the highest ideals themselves, are created in human minds by an actual, existing process such as we above described, then the value of an ideal must be tested by its relevance to this process and by the effectiveness with which it guides conduct in providing conditions which permit this process to operate most effectively. Since this is our claim, ideals are subject to criticism and correction by knowledge gained by science concerning the conditions required for the most effective working of this threefold process.

If values are indefinable essences accessible only to the intuitions of unique individuals, it still is the case that these intuitions are more likely to occur under some conditions than under others. Thus the problem for science remains with all its unimpaired urgency and importance, namely, to discover the conditions under which such intuitions will occur.

If values are eternal Platonic forms of Ideas, the problem still remains,

as Plato well saw. The problem is to organize society and education so that men can envision these Ideas and shape this world most nearly into their likeness. This problem must be solved by scientific inquiry.

Last of all look at the attack upon application of science to values as it is set forth by Aldous Huxley in his *Brave New World;*[5] by W. McNeile Dixon in *The Human Situation,*[6] and by William James in *The Will to Believe.*[7] The rejoinder to these statements is very simple. Whether the writers mentioned, and many others that might have been, so intend or not, they are only saying that some of the conditions allegedly sufficient to render situations humanly enjoyable are not truly so. Certainly the dull uniformity and regularity of Dixon's "universal reason" would be unbearable. So also with James' "white-robed, harp-playing heaven" in contrast to this "delicious mess of insanities and realities." But whether the writers know it or not, none of these statements are opposed to the claim that scientific inquiry can discover and help establish the conditions which render situations cumulatively enjoyable, mutually sustaining and mutually enhancing, with innovations, problems and freedom sufficient to quicken all the powers and all the vividness of felt quality that man can endure.

---

[5] Garden City, N.Y., Doubleday, Doran & Co., 1932.
[6] New York, Longmans, Green & Co., 1937, p. 190.
[7] New York, Longmans, Green & Co., 1897, pp. 168–169.

THE DIFFERENCE BETWEEN KNOWLEDGE AND VALUATION

HERBERT FEIGL

VALUATION, preference and choice are characteristic of all behavior that is propelled by needs and interests, and controlled by learning and ingenuity. Whether such goal-directed behavior occurs in man only, or also in some other species of animals, is either a question of definition or a problem for empirical research. Neither requires discussion here since the issue under consideration concerns human valuation and human knowledge. It will be sufficient to remind ourselves, and—in our age of science—it should scarcely be necessary to argue, that valuation, in the sense indicated, is a legitimate and important subject for scientific inquiry. Significant contributions toward a descriptive survey as well as toward a causal analysis of human wants, interests, ideals and appraisals have been forthcoming from biology, psychology, cultural anthropolgy, economics, sociology, etc. Valuations and the contexts in which they occur and evolve, belong to the *factual* subject-matter of science. If it is a fact that, for example, the people of Alor extol strife and pugnacity, while Quakers extol peace and conciliation, then social science can investigate the conditions that engendered these or other value attitudes. Social science can also explore the likely consequences of value-attitudes upon various aspects of individual and group life. A denial of the possibility of scientific inquiry into valuation reveals ideological commitments of a highly obsolete if not obscurantist sort. It may even be conceded that the insistence upon a Dualism of Fact and Value is with many (but by no means all) philosophers an expression or a projection of typically theological or transcendentalist convictions. Thus far, it seems, I heartily agree with Professor Geiger. If there is any serious disagreement, it concerns only the question of an adequate account of the relation between Knowledge and Valuation.

In the spirit of the candor, clarity and consistency so essential for the scientific outlook I shall have to insist upon a few distinctions and a few searching questions.

It is one thing to *study* evaluations, their conditions and consequences; it is another thing to *make* evaluations and in this manner to influence the course of events (even if this influence affects only one's own attitudes). It is one thing to *de*scribe by means of declarative statements what *is* the case, or to predict what will (probably) be the case if certain conditions are fulfilled; it is another thing to *pre*scribe by means of overtly or covertly imperative sentences (suggestions, recommendations, commands, requests, propaganda, solemn resolutions, etc.) what *ought* to be done. It may be cheerfully admitted that this distinction is drawn primarily for the sake of logical clarity. We have already stressed that the very acts of deliberation, choice, preference, decision, etc. that lead to active intervention with exist-

ing conditions are susceptible of scientific analysis. But if we are not violently to distort the common conception of science and thus to incur intolerable confusions, we must recognize that science is the search for *knowledge*. And the most important results of the sciences are formulated in laws that tell us what happens (with what probability) under what conditions. This sort of information, be it as crude as that of primitive man or as intricate as twentieth century theoretical physics, is of course *applied* in the most various practical concerns. The term "applied science" is somewhat ambiguous. It may refer to such *activities* as the constructing of buildings, the making of drugs, or the enacting of measures of social or political reform. It may also refer to the *cognitive content* of such disciplines as mechanical engineering, industrial chemistry, medicine, or social work. "Applied science" interpreted in this second sense consists in a body of knowledge, selected from pure science and focused for application in the first sense.

One of the characteristic features of science (in the sense of pure knowledge and the search therefor) is the possibility of removing disagreements concerning matters of fact. Observation, measurement, experimentation and statistical analysis, together with generally well agreed upon methods of interpretation and inference, provide at least in principle the means for the adjudication of scientific disputes. The history of psychology within the last thirty or forty years, for example, shows rather convincingly that superficially extremely divergent theories actually differ much more in jargon, and partly in the selection and emphasis of their subject-matter, than in their substantial cognitive content. Jargons are intertranslatable; bias and predilection are themselves capable of scientific detection—in the psychology and sociology of knowledge. In short, the methods and techniques of the sciences provide a basic platform of agreement. Science, at least nowadays, is conceived as a social enterprise. Everyone, equipped with the requisite intelligence and instruments, is in a position to examine the validity of assertions or hypotheses regarding matters of fact. This is the essential meaning of the *objectivity* (better perhaps, "intersubjectivity") of the scientific method. Since we are here discussing questions of principle, we need not be concerned with the notorious practical difficulties of the decision between rival hypotheses. Such decisions depend on considerations of the weight of incomplete evidence and can be just as vexatious in astrophysics as in cultural anthropolgy. It is also not denied that the very adoption of the scientific frame of reference is a matter of value-decision. Nor do we dispute the patent fact that the results of scientific knowledge have had and continue to have tremendous repercussions in social attitudes and evaluations. Finally, we admit the obvious mutual fertilization of pure and applied science.

What I do dispute here is the claim that a science of valuations *ipso veritate* provides a validation of valuations. I dispute this for two reasons, both very simple and obvious, the second being the more fundamental one.

(1) Since any application of scientific knowledge presupposes purposes and since it is in the light of such purposes that we evaluate means (actions and instrumentalities) we cannot ignore the actuality (or at least the possibility) of cognitively irremediable disagreements in regard to purposes. This implies that there is nothing in the nature of evaluation that ensures its intersubjectivity. Here then is one essential difference from scientific inquiry. Lest this point be misunderstood, let it be clear that we are here not concerned with the difficulties of reliable knowledge concerning conditions and consequences. This sort of information is needed as the cognitive basis for determining which means lead to the attainment of which ends, most expediently and with the least interference with other ends-in-view. Empirical research is in principle capable of resolving this kind of problem, even if in practice we are often all too painfully ignorant to answer such questions with a sufficient degree of assurance. In contradistinction to issues of this sort, which are in principle resolvable by the scientific method, it remains possible that no matter how complete the information of each party to a dispute over *ends,* no agreement on these ends may be attainable. It is at this point where so-called "rational" (i.e., logical and empirical) considerations invariably give way to persuasion, compromise, separation or coercion. While these procedures are, of course, susceptible to scientific study, they are not cognitive procedures (directed at the disclosure of fact) in and by themselves. (2) Even if there were complete unanimity as regards purposes, i.e., no single conflict as regards ends (a truly utopian assumption!), it could not be argued that scientific knowledge by itself is in a position to furnish evaluations as to the best course of actions, i.e., to furnish directives as to what ought to be done. Scientific knowledge, even if its concerns the conditions and consequences of valuations, is in a perfectly clear sense value-free or value-neutral. Science can study the origin and the effects of directives; it is, like any other intelligent activity, guided by directives; but the conclusions of science, while instrumental and increasingly indispensable for the justification of the cognitive grounds of directives, can never be directive in and by themselves. The body of scientific knowledge consists of *information,* no matter whether it concerns the energies of inorganic nature or the motivations of human individuals in the social context. The stamp of approval, the accent of endorsement, the force of encouragement, the factor of exhortation, in short the motivative appeal is something that is part of the life process itself, but not in any way contained in the cognitive content of the propositions of science. Information may be utilized by goal-seeking, intelligent agents, but it is precisely the needs and interests of these agents which give rise to the imperative character of valuations. "If you want to preserve your good health you must eat food containing (such and such) vitamins"—this is a conditional imperative, based of course on scientifically ascertained knowledge. But the scientific information itself contains only statements concerning what consequences regularly attend the use of various foods. Scientific statements can

by their very nature not contain such words as "advisable," "desirable," "should," "ought," or "must."

In the inorganic sciences there is scarcely an opportunity to neglect these important points and thus to beget confusions. But it is notoriously difficult to avoid value-charged terms in the social sciences. "Progress" and "reaction," "advance" and "cultural lag," "reform" and "stagnation," "justified measure," "inexcusable action," "normality" and "aberration" (or other equivalents of the bald adjectives "good," "bad," "better," "worse," "just," "unjust," "right," "wrong," etc.) occur almost unavoidably even in the most academic treatise of economics, sociology and history. Just because, as an empiricist and naturalist, I insist on the possibility of a scientific investigation of the facts of valuation, I must urge a scrutiny regarding the very meaning and validation of value judgments. If it is purposes (flowing from needs and interests) which justify valuations—then *whose* purposes establish the vindication in a given case? If it is interests which give rise to value judgments, whose and which interests are to take precedence over which others? Unless we frankly note the facts of conflict among purposes and interests, and unless we explicate the principles by which we claim that such conflicts can be "rightly," "justly," or "beneficially" settled, we have not even begun to approach the problems of valuation as they confront us in the stark realities and in every dimension of human life. The issue becomes especially clear and poignant in connection with moral value judgments. Even the most complete and reliable knowledge regarding the consequences of various (contemplated) courses of action will not yield evaluation unless those consequences are examined in the light of moral norms. Far from being anything supernatural or metaphysically transcendent, these norms (or more precisely, their origin, embodiment in judgment and motivative function) can be accounted for as resultants of a long process of social interaction and evolution. In the give and take of interpersonal relations, in the claims and counter-claims made by man upon fellow-man, certain ideal standards, e.g., of equity and benevolence could not fail to arise. I say advisedly "ideal standards," since their frequent violation in practice is as obvious as is their role as principles of moral justification and criticism. There is of course a distinction between moral and non-moral evaluations. If we evaluate the possible choices from a restaurant's menu exclusively in terms of our preferences of taste, then there is no moral issue involved. But if our choice in that situation is (at least in part) determined by a consideration for the welfare of our neighbors (as in a period of scarcity of food), then we accept clearly the jurisdiction of such moral principles as "Act unselfishly" or "Do not arrogate special privileges to yourself." If the philosophical analysis of moral norms in its long and tortuous history has contributed one important clarification, then it is this: The plausibility of the derivation of ethical principles from a factual account of needs and interests and the means toward their fulfillment is altogether specious. Such alleged derivations reveal themselves

either as *non-sequiturs* (for indicatives never imply imperatives), or as *petitiones principii* (for the interest assumed is already tantamount to a moral ideal). The outstanding criteria of morality of the people(s) we call civilized, may well be summarized under the three headings of Justice, Benevolence and Perfection. Only if these ideals are included in the definition of such terms as "(genuine) satisfaction," "(true) happiness," "the (good) society," "(mature) human nature," etc., does it appear as though we could derive those moral standards from the facts of our needs and interests. But such definitions (as indicated by the bracketed value-epithets) must be recognized as "persuasive" in character.[1] That is to say, such definitions perform not merely the usual task of an explication of meaning but provide also the motivative impetus needed to connect the purely cognitive with the valuational role of the term in question.

We conclude that the view according to which Scientific Method applied to concrete issues can by itself yield value judgments is untenable. Eager to wrest the authority of moral judgments from the dogmatic priesthoods of theology and metaphysics, John Dewey and his disciples have rightly attempted to relocate it in Creative Intelligence. But the identification of creative intelligence with science (or even with scientific method) involves the very confusions which we were concerned to dispel. "Intelligence" just like its older, but in some quarters unfashionable, synonyms "Reason" or "Rationality" is apt to connote much more than adherence to the principles of deductive and inductive logic. Intelligence and Reasonability usually comprise also the moral principles of justice, impartiality and non-violence (in the settlement of conflicts of interests). One may share these ideals of the good life in a social democracy (I certainly do!), but one need not for this reason succumb to the temptation of (fallaciously) basing these ideals upon the results or the methods of science.

To forestall what I trust is an unlikely misunderstanding of the foregoing argument, I wish to emphasize that with no word in these remarks have I tried to advocate a practical separation of pure and applied science. I have merely pointed out an important distinction. It must be left to experience to determine to what extent the gathering of reliable knowledge in the social sciences may be accelerated (or else retarded) by what degree of concern with immediate practical applications.

While the distinction between knowledge and valuation was here insisted upon primarily for the sake of logical clarity, it must not be assumed that it therefore is of use only inside the alleged ivory tower of "pure analysis." As long as we subscribe to a policy of the open mind—and is not this one of the most essential features of creative intelligence?—we stand ready to revise both our knowledge-claims and our valuational attitudes.

[1] For a detailed analysis of persuasive definitions, as well as important related issues, cp. C. L. Stevenson, *Ethics and Language*, New Haven, Yale University Press, 1944. See also Stevenson's essay "The Nature of Ethical Disagreement," in *Readings in Philosophical Analysis*, edited by H. Feigl and W. Sellars, New York, Appleton-Century-Crofts, 1949.

It would seem indispensable to be able to distinguish clearly between the two, just because in any concrete issue we shall have to know which is which in order to find the proper point of application for criticism and reform.

Note: A more detailed analysis of the problem of the meaning and the limits of justification in ethics as well as in science is the subject of my essay: "De Principiis non Disputandum . . . ?" published in the volume *Essays in Philosophical Analysis*, ed. Max Black, Ithaca, New York, Cornell University Press, 1950.—The delimitation of the scientific from non-scientific endeavors is a major topic of my article "Naturalism and Humanism; An Essay on Some Issues of General Education and a Critique of Current Misconceptions Regarding Scientific Method and the Scientific Outlook in Philosophy," *American Quarterly*, Vol. 1, No. 2, Summer 1949, University of Minnesota Press.

CHOICE AND DECISION IN SOCIAL INTELLIGENCE

R. Bruce Raup

One of Professor Geiger's sentences provides a text for my statement. He writes that "... the very methods of intelligent inquiry have been excluded from prosperous contact with whole segments of human experience, those of human decision ...." Standing at the point of vantage afforded by these words, it is possible to point out both the wide range in which I find Professor Geiger's argument profoundly satisfying and also a crucial point at which it presents a difficulty. The sentence highlights the argument (1) that scientific method can come into effective relationship with values only as its hypothetical temper and its experimental, operational test come to be realized in the processes of human choice and decision, and (2) that this imperatively needed relationship has been, and is still, blocked by the persistent and institutionalized conjuring of absolutes and mystical certainties, especially when the going gets hard and nerve begins to fail.

In the pages which follow I want to add emphasis to the conclusion that scientific method can come into full play in relation to values only as decision and choice become the focus of methodological attention. And yet, it will be necessary to point out what I believe are certain limitations and other difficulties in the way Professor Geiger, along with others today, believes this extension of method can be effected. Similarly, while there is evidence sufficient that the two-world, extra-experiential, "extramural" recourse of despairing minds has been bred into our culture and is thus an institutionalized block to the rapprochement of science and social understanding, this is by no means the only, or today, even the most serious such block. The latter is to be found, in part at least, in the limitations of our most cherished conception of method. The issue is not primarily between stalwart devotion to *the one* adequate method and timid resort to mystical and anti-rational beliefs. It is rather between less adequate and more adequate conceptions of the method of intelligent direction and control.

There is so much in Professor Geiger's paper which is acceptable that emphasis upon difference may be misleading. This should be avoided, for my statement will assume much of his position and build upon it. Prominent among such bases of accord are: the belief that science and values are in one universe of discourse, as opposed to two or more; that persistence in the separation of fact and value is headed for social disaster; that scientific method is itself a representation of certain social values which are promoted when it is promoted; that control of bias means not sterile neutrality but more intelligent preferences, convictions and decisions. All of these facets of a position are either expressed or implicit in his paper, and with him I believe them all. To deny them would, for many today, be a serious step backward. The indicated problem arises when we ask where and how we shall go from here. And my proposal is that this is primarily an issue among

those who believe basically as does Professor Geiger and not primarily between these and the philosophical dualists and mystics. Obviously the answer to this problem of where and how to go forward is not clear, and we might well be open to whether the difficulty may be in our own predominating conceptions. I believe that, in no small part, it is, and that it focuses in our conception of method.

In a word, our practically exclusive preoccupation has been with method as it pertains to the ascertainment of fact, and this is neither a necessary nor a justified limitation. And, persisted in, it constitutes a, if not *the*, most serious block to the development of effective intelligence in human affairs.

Professor Geiger rightly deplores the "fact-value dualism" which marks our time and which only recently has been breaking under pressure of dire public circumstance. But how are fact and value to be brought under one discipline and dealt with in their integral relationships with each other? His answer, I must conclude, is that they shall both be brought under the discipline of the judgment of fact. For the method of science, as he sets it forth, with its hypothetical approach and its goal of verification, is focally the method of establishing descriptive and explanatory facts and generalizations dependable for prediction. George Lundberg is quoted to this same effect, as he sees "if-then" relations serving as the central pattern of concern in the method of judgments of practice. This argument has a long and a persistent following. As far as it goes, I believe in it. And it goes far—but not far enough, and not entirely in the right direction. It is method in an essential function of human intelligence being taken as the only and exclusive method for all functions. It is assumed apparently that all other functions are disciplined and directed in and through the method of this one. At any rate, no generalizations of method pertinent directly to other functions are advanced. The latter are in effect left as a-methodological in character, except indirectly in their aspects of fact.

It is correct, as stated, that even "the act of choosing can become a matter for investigation," as can also "degrees of approval and disapproval," and decision, and any number of other elements in value judgments. The investigation contemplated by Professor Geiger however is a factual investigation. Again, so far so good. But a thorough factual analysis of choosing or deciding or of any other focally valuational act reveals that it is itself not just a factual judgment and yet that it is a judgment. It is a function in intelligence. While it does have "if-then" components, its mental operations are not limited to these. There are also commitments, considered preferences, zeals, competing goals and objectives, unique individuals or groups seeking outcomes congenial to themselves, and many other preference-pointed, participant elements. Any one of these could have an "if-then" phase and might be taken aside for examination of its factual dependability. But in the process of choosing or deciding, the factual investigation is quiescent and the interacting preferences move toward a formulation which

will release a course of action, a course primarily of commitment, not just a course which is seeking to *find out* the facts.

My investigation of choosing and deciding reveals that as functions in active intelligence they are either to be honored as worthy of examination in their own distinctive character for their own distinctive principles of method, or else to be regarded as largely uncontrolled melees in which biological and personal habits and impulses, conditioned only more or less separately from each other in their "if-then" aspects, carry on a jungle warfare for domination. The insistence upon crowding the conception of method for all functions through the cold and limited channels of the method of determining fact can leave us only this latter recourse. And it sounds disturbingly like what has actually been happening. We might be justified in this if nothing further were available to us. This partial method is, we trust, better than no defined method, although even this has yet to be inclusively demonstrated. But there is basis for the confidence that we are not so limited. The clues to the issue as they appear in Professor Geiger's paper have therefore been followed up. These lead me to emphasize the need for a thorough and sustained study of method in decision, choice, and all related judgments of practice.

There *are* possibilities of method shaped directly to valuational acts—decision, choice, etc. Statements to this effect have been written and published.[1,2,3,4,5] But, in agreement with Professor Geiger, the achievement of what is needed awaits "the cooperative activities of research workers and creative thinkers in all areas where the very data for investigation are human choices." I will repeat that my statement seeks to move forward with the enterprise thus pictured. It would add, however, that the prospects of success will not be bright until we give full weight to the need for generalizations of method which bear *directly* on the only judgments in which values are fully and actively alive, i.e., choice and decision.

One suggestion for the guidance of positive development of valuational method comes from examining another of Professor Geiger's sentences: ". . . . scientific method is not 'merely' a means to some extramural end, but. . . . contains within itself a dynamic and self-correcting purposes for which no ethical apologies need be made." This is a most significant statement and I accept it, with only the qualification that the moral good represented in and through scientific method is not necessarily limited to this as the one and only mode of its representation. For, what is this moral good? It has, I believe, two complementary components: (1) the eventual determination by the human community itself of what is to be held true or good; and (2) respect for individual persons as free and active participants in the processes in which their truth and their good are conceived and worked out. Whatever else scientific method may be, the denial of either one of these facets in its moral foundation would mean its eventual collapse. These values did not arise solely through the method of establishing fact. The reverse would be more nearly the case. Nor do they express themselves effectively

only through this method. It is my proposal that they can come into fruition only very partially by way of method in this one function; that they enter more fully into intelligent human enterprise through methods which guide the functions wherein people are most fully alive, that is, through methods pertinent directly to personal and social decisions and choices.

In these humanly more inclusive functions the operational test of validity is still pertinent. For what basically does it mean to say that the formulations of ventured findings must "carry with them the means for their clarification as well as for their justifications or rejection"? Basically, it assumes that confirmation is established only in the community of those who observe. The words "clarification," and "justification" point unmistakably to this conclusion. The operations contemplated are means to this end just because they confront participants with what they can actively, knowingly, and commonly experience. When the question before people is a question of fact, the ventured form of solution, the hypothesis, in order to be confirmed, must carry with it operational meanings which would require people to note and conclude that the proposal is descriptively correct —correct whether or not they like, or prefer or have been committed to that which is thus established as correct. The community pertains only to the descriptive correctness of the proposed conclusion or generalization of fact. But when the question concerns *what should be done,* the community which will confirm any proposed resolution is involved in a manner which, if it were given play when the focal purpose is the ascertainment of fact, would corrupt and discredit the latter process entirely. For the issue of *conduct* is one precisely in which there is deliberate effort to find a more, rather than a less, commonly satisfactory and committed formulation of a proposed course of action. This is a process in which not just the facts but, far more, those valued elements which the facts are about come into active operation in relation to each other in the quest for a resolving and action-releasing formulation.

This process of decision and choice is no less reflective and deliberate than is the process of venturing and establishing findings of fact. The two functions are integral parts of human intelligence. They are mutually dependent and mutually directive and corrective. But they are not one and the same function and, in the pursuit of adequate method, they cannot be substituted, one for the other. Their mutuality is served through alternation of emphasis and pursuit, not through identification.

The operations through which a proposed decision or choice is mediated toward community of acceptance or rejection must be in terms of active relationships in which the persons concerned may find a community of preference. In striving toward this community, pertinent facts define some confirmed limits and some possibilities that can be depended upon. But the process thus *conditioned* by established fact is one in which preferences and commitments are working among themselves toward a form in which, as a product of their mutual influence, there is effected a common commitment to action or to a principle or type of action. The operations through

which community of understanding is thus represented or achieved are as truly confirmatory as are those through which factual findings are established. For in both cases it is basically the community persuasion which confirms. The value-pregnant actions which are contemplated in making convictions clear and common can be reflectively experienced and can mediate a deliberate community.

It will entail no less importance for the pursuit of fact when we come to realize that its method is only an integral and indispensable part of the full method of intelligence. As such it serves the basic ethic of human community and personal freedom. But when it becomes a usurper, claiming to be the all-inclusive discipline of human intelligence, it does defeat the very goods of which it is one representative. For it precludes the methodological development of the functions which, both more basically and more amply than it, articulate intelligence toward human good.

It is my belief that sustained recognition of this prior claim of the method of human communication (the advancement of the moral community) would go far toward bringing science into moral perspective and toward releasing and schooling the intelligence of people for the achievement of greater humanity in their social arrangements.

[1] Raup, R. Bruce; Benne, Kenneth D.; Axtelle, George; and Smith B. Othanel, *The Improvement of Practical Intelligence*, New York, Harper and Bros., 1950.

[2] Raup, R. Bruce, "Method in Judgments of Practice," *The Journal of Philosophy*, 25: 801–817, 1949.

[3] Benne, Kenneth D. and Muntyan, Bozidar, *Human Relations in Curriculum Change*, Springfield, (Illinois Secondary School Curriculum Program, Bulletin No. 7, Circular Series A, No. 51.) Illinois Dept. of Public Instruction, 1949.

[4] Burtt, E. A., *Right Thinking*, New York, Harper and Bros., 1946, Part IV.

[5] Axtelle, George and Raup, R. Bruce, "Some Working Terms of the Needed Discipline," *Teachers College Record*, 46: 213–218, 1945.

ACTION-RESEARCH AND THE VALUES OF THE SOCIAL SCIENTIST

RONALD LIPPITT

I BELIEVE Professor Geiger has provided a very thoughtful framework for describing and clarifying the role of the social scientist in action-research projects. With some minor variations in usage this term "action-research," coined by Kurt Lewin, has continued to denote the activity of applying research methodology to the diagnosis and solution of practical problems of taking action. I think the term "operational-research" is used in pretty much the same way to stress the notion of applying scientific methods to the solution of operating problems, i.e., problems of acting effectively, of making good action decisions.

Professor Geiger has stressed that values are the resultants of decisions to act in one way rather than another in situations of competing interests. These resultant preferences or values then continue to operate as direction setters in future decision situations unless there are re-evaluation processes leading to the modification or "reconstruction" of these values. It is suggested in his paper that the application of the spirit and technology of science to the assessment of situational realities and consequences in these preferential judgment situations would result in a continuous process of value construction and reconstruction, and should result in values which are more in harmony with the basic requiredness that in the long run the organism must solve its problems successfully or suffer unpleasant consequences of pain, frustration, or even extinction. Or, to invoke the treacherous means-end model, it is suggested by Professor Geiger that the stable end values of the person or group should be methodological in character, i.e., 1) that working on the solution of problems (taking action) is good, and 2) that using the intelligence-gathering methodology and consequences-testing methodology of science in the discovering, defining, diagnosing, and solving of these problems (making value decisions) is good. There is certainly much more to his statement, but this seems to me to be the central theme.

I'd like to examine, in this framework, some of the problems of the social scientist who is faced with the decision of whether or not to participate, and how to participate, in action-research with some action group in our society. The invitation probably comes from some group that wants to become "more scientific" in evaluating its program and making better policy decisions. For this social scientist a number of the philosophical issues raised by Professor Geiger become concrete practical realities. For example:

1. Some of his social scientist colleagues ask him if he realizes that his collaboration in this practical problem-solving business really means that he is tender-mindedly stepping out of his role of scientist. Perhaps if he

just helps them collect a few facts to diagnose their problem he will be safe, but if he gets mixed up in making recommendations about what should be done then he is out of bounds and will lose his cloak of immunity as "neutral" and "objective."

2. Some of his friends who are more interested in action than in neutrality assail him with the charge that he is working with the wrong group. His technical skills are "being used" by the anti's" to further their value position when he really ought to be siding with the "pro's" instead.

3. Other colleagues assure him that it is perfectly all right for some social scientists to devote their energy to helping lay groups solve practical action problems, but after all that's pretty superficial (i.e., low status) stuff compared with working on basic scientific problems which arise from theoretical explorations.

After reading Professor Geiger's paper our social scientist thinks through his decision problems along these lines—

1. It does seem to me, he thinks, that the conception of discovering "knowledge for the sake of knowledge" is too narrow a conception of my personal end-values as a social scientist. Instead I agree with Professor Geiger, as I understand him, that it is desirable that scientific knowledge, "the scientific attitude," and the technology of scientific methods be applied to the defining and solving of problems by all individuals and groups in our society. I understand, but cannot accept for myself, the viewpoint of some of my colleagues who believe that these problems will be better solved in the long run if all energy is devoted to "basic research" now.

2. I also agree with Professor Geiger, he continues, that there is quite a strong tendency in many persons or groups to divorce the scientific attitude of open mindedness in sensing and defining problems from the scientific technology of collecting and analyzing facts about the realities of the problem situation.

3. Knowing what I do about the data on the psychology of learning, I have a hunch that one reason for this unfortunate split is that most individuals and groups have not had the opportunity to learn what scientific problem solving is by actually participating successfully in such a process—at least not in the area of solving problems of human relations. Therefore they continue to think in terms of using the tools of a scientist-technologist (from outside) rather than in terms of participating collaboratively in the process of scientific problem exploration and solution. I know also that a store of relevant facts cannot contribute functionally to decision making and action unless they are thoroughly accepted and understood by the individual or group making the decision.

4. Another fact I know (continues our scientist) is that many individuals or groups are not aware of the actual problem situations that are blocking and frustrating them. For a variety of reasons these problem situations remain unperceived or undefined. Therefore, the group needs to use methods of getting data which will reveal unperceived problems as

well as to use methods for analyzing problems to which they are already sensitive.

5. In view of these facts, and in view of my value of having more and more persons and groups utilizing scientific attitudes and methods, it seems clear to me that one of my proper roles in action-research must be that of a teacher or trainer in "the scientific approach" rather than merely a guest collector of data of the type prescribed by the current problem awareness and problem definitions of the group. It seems obvious that I am also going to have to be a very active learner, as well as teacher and operating research methodologist.

6. Now let me turn to this particular decision situation a bit more concretely to see where this line of thinking leads. The actual group which has asked for help is a community agency which has just had a conflict with the community chest over budget and has withdrawn from the chest, planning to raise its own funds. They want me to help make an attitude survey to reveal how the money raising campaign can best be promoted. From my brief contacts with this group I have a hunch (continues our scientist) that the strong end value of achieving a certain financial goal on its own is preventing the agency leaders from being sensitive to several other interdependent parts of its problem situation, e.g., the serious split in the staff over the policy decision—which will affect any attempt of the group to decide or act in any direction, and the feelings of the community about this deviate agency. These problems of relationship within the group and in its intergroup relations will become perceived as parts of the problem to be solved if I as the consultant scientist can communicate an open-minded scientific attitude and get acceptance of an adequate methodology of problem assessment rather than being content with the single role of survey technician. But isn't this an unrealistic requirement for me to hold out for in the first stages of such a working relationship? Perhaps so. The unfolding of a training relationship is often even more gradual than the stepwise development of an appropriate research strategy. Unless I accept this training responsibility as part of my scientist role we can expect little bridging of the gap referred to by Professor Geiger as "the differential lag between knowledge and action."

7. But now I see glimmerings of another knotty problem which may very well develop in this process of interaction between crystallized value directions and a methodology of re-evaluation. What if I am able, as I work with the group, to get them to accept a wider perspective on the definition of their problem, so that we collect data on relevant aspects of staff morale and community relations, as well as on fund solicitation potentialities? Then what if the staff refuses to consider the data, or distort them greatly in the decision-making situation because the findings really push for a modification of the central value of "going ahead on our own?" Or what if the group interprets a portion of the data correctly as evidence that the agency can achieve its immediate money-raising goal, although it is also clear that this will create quite a few problems for other groups during the process of

solving the immediate problem of this particular group? It seems clear that Professor Geiger is quite correct in saying that "scientific objectivity cannot be amoral in the sense of not giving a damn" in such a situation as this, but in what direction does my responsibility lie?

8. My mind runs back to the thought that my responsibility is to the total community as well as to the subparts of it. Certainly if any group rejects rational problem solving in its relations with other groups in the community, then this is a problem of concern to the total community of closely related groups. It is my responsibility then to help the larger group become aware of this problem if I am able to do so, in order that the problem may be faced by the community of groups. This is very much the same responsibility as that which goes with discovering that a scientist colleague has intentionally destroyed, withheld, or falsified data which are relevant for scientific progress. By becoming aware of the problem the larger community is in a position to promote and uphold methodological standards of problem-solving integrity.

9. This line of thinking leads me to the hunch that an extension is needed to Professor Geiger's value that "the solving of problems is good." Perhaps the insensitive creating of problems for others is also bad. The frequent appeal to the end value of competitive survival for the individual or subgroup seems inadequate. Creative contributions to the survival skills (i.e., problem locating and problem-solving skills) of other individuals and groups seems to be a rather important notion. Our scientist suddenly realizes that this seems to be a rather important personal value for him, pushing him toward deciding to spend some of his time collaborating in action-research projects as well as working on the production of more generalized "basic" knowledge.

This is all still pretty fuzzy, but our social scientist decides to act—to accept the invitation of the agency to participate in an action-research relationship. He sees that if he is going to learn something about this problem of the functional relationship between value and science he is going to have to follow good scientific procedure, i.e., he is going to have to expose himself to the phenomena he wants to investigate—as a next step in constructing hypotheses for further study. He recognizes now the validity of Professor Geiger's final point that much laborious scientific work is going to have to be done on the problem of decision he has been struggling with. But one has to act in order to begin working on the problem.

After his first plunge our social scientist summarizes his tentative position to his three types of critics somewhat as follows:

1. To colleague number 1 who thinks action-research is an unsophisticated headlong mixture of scientific techniques and social reform zeal.—Yes, I'm quite ready to agree that you are pointing to a tough problem. In many respects it is even tougher operationally than it is philosophically. I find it quite possible to stay within what I consider appropriate scientific roles of teacher of scientific approach and method, research methodologist,

and learner of new facts about social process and social structure. But I have practical difficulties doing a skillful job of defining this role clearly for those groups I work with who at first want me to demonstrate that I accept their existing values, as proof that they can trust me. It's hard also to keep from being perceived as partisan by other groups who would like similar help but perceive I have taken sides. And of course it's hard for me to interpret my role to you because you haven't shared in the blow by blow experience of what an action-research relationship really is and how it can be handled. I'm not clear whether or not we differ in the acceptance of methodological goals as the criterion values for ourselves and for others. If we accept the idea of successful problem solving and survival as the basic framework, then I think you would probably agree that I can stay within appropriate scientific roles when I participate in action-research. You might not agree on my feeling of obligation to communicate methodological skills to other groups in order to contribute to the probabilities of their successful growth, survival, and contribution.

2. To friend number 2 who feels I'm helping the wrong side—I'm sorry the other group didn't approach me also. Ideally I'd like to be collaborating with both groups because evidently part of the problem lies in the relationship between them, and, therefore, this relationship needs to be studied by both groups as one aspect of the problem to be solved. If I can manage it I'll take the initiative to work with "the other group" also. I am not interested in strengthening and promoting the existing value structure of the organization I am working with. On the contrary, I have an interest in getting them to take an active interest in adopting methods for continuous study and evaluation of the consequences of acting in terms of their values and discovering needed revisions in these values. If, after an adequate trial period, I become convinced that they are unwilling to be receptively open-minded about discovering what their problems are, or unwilling to consider the implications of the data collected by appropriate scientific procedures, then I will feel obligated to sever my relationship, unless they kick me out first. I will also feel obligated to inform other groups who are interdependent with this group of my discovery of their unreadiness to operate in terms of rational principles of problem solving. I feel it is important to do this so that appropriate intergroup standards of mutual responsibility can develop and so that social pressure can be exerted on such deviant groups.

3. To friend number 3 who tells me that this type of scientific effort is legitimate but unproductive and low status—It is qute probable that the methodological sophistication of the work I do in these action-research projects, and the genotypic generalization range of the data, will be inferior to the work you do, or I do, in more highly controlled experimental settings where our problem springs from previous theoretical development and represents a planned step in the stepwise development of a field of knowledge. On the other hand, there are certain areas of needed basic knowledge which are most accessible for clinical observation and systematic research if one gets immersed in this process of intense interaction between values, decision

situations, and action. And, lest this sound defensive, I must also disagree with you a bit about priority values. The acquiring of scientific problem-solving skills and values by more and more individuals and groups in our society is a priority value for me—not only for the sake of those groups but in the interests of the survival of science and scientists as valued elements of the culture.

SOME REFLECTIONS ON EMPIRICAL INVESTIGATIONS OF ETHICS

NICHOLAS HOBBS

PROFESSOR GEIGER'S arguments are cogent. They hit us where we are weak, and our will to resist is not great because our hearts are not pure. In our hearts we believe that values are important, that directions and commitments make a difference, and we find no peace in asserting that the consequences of our scientific endeavor are of no concern to us. The atomic scientist with his bomb is in no greater travail than the psychotherapist with his quandary of adjustment—but adjustment to what? Each scientist in his own field finds thorny questions of act and consequence. Today few can hold to the simple value that the "pursuit of truth" is the only commitment essential to the scientist. Our lack of certainty over what our professional values should be makes us susceptible to an argument that we can have control over value systems as well as over servo systems. If science, which got us into this mess, can also get us out of it, let's not be wasting any time.

Knowing how treacherous is the desire to believe, however, we should perhaps mobilize some resistance to Professor Geiger's thesis, before giving in to it. At least we may be able thus to identify some trouble spots, some problems that must be solved before much headway can be made in applying scientific method to the study of values.

As is so often true in psychological research, we quickly run into criterion difficulties when we contemplate investigations of value systems. "Good" and "bad," "desirable" and "undesirable" rise up as critically important but most slippery concepts. We will do ourselves no good in minimizing the difficulties which these central terms will bring forth when we attempt to define criteria for ethical studies. Not the least of the difficulties will arise from the fact that we have for centuries left their definition to philosophers, to priests, novelists, poets, parents, and our own unsystematic interpretation of experience. Most of us would disclaim as pretentious any authority to establish standards for "right" and "wrong," though indeed we confidently make choices as though we were sure of the ethical validity of our choosing. The large and awesome terms of "right" and "wrong" may ultimately be broken down, as Professor Geiger suggests, into simple matters of choice. We have not hesitated to attempt quantitative statements of determinants of simple choices, at least in animals; and we have gained some facility in genetic descriptions of "choice of symptoms" or of affinities for certain modes of adjustment, in clinical work. But even so, choice is no simple problem, even in the simplest situations we can devise. Buel [1] has identified "ninety-three possible factors which have been or may be advanced as

[1] Buel, J., "Differential Errors in Animal Mazes," *Psychological Bulletin*, 32: 67–99, 1935.

causes or predictive concepts in the determination of differential errors" in maze problems. One of the ninety-three factors is chance. In embarking on such an enterprise as defining criteria for value-judgments, we should at least realize that there is more to the task than insisting that it can be accomplished. Perhaps it can. But we should also be prepared to accept Lundberg's conclusion that "science as such is ethically neutral," and that social scientists should "avoid attempting to endow themselves . . . with authority to tell man (a) what he should want as well as (b) how to achieve his goals." [2]

Lundberg simply warns against accepting hypotheses regarding ethical behavior as established conclusions, and suggests a more limited role for science in the solution of the problem of values than does Geiger: "The great contribution of science to ethics should be in providing man with a more reliable record of interpretation of his experience." Lundberg implies that the central problem of criteria for "right" and "wrong" will continue to elude scientific definition, except as hypotheses to be tested.

Professor Geiger attributes the reluctance of social scientists to undertake investigations of problems of value to cultural lag, to the failure of our institutions to keep up with our theoretical advances. I am not sure that this is an adequate identification of the source of our reticence. Granting some reluctance of scientists to get involved in problems of value, there is the possibility that the chief deterrents to scientific exploration of values are methodological in nature. Psychologists and sociologists exploring attitudes toward war, capital punishment, the Negro or the Chinese, have shown a willingness to examine one aspect of problems of values. Tests of interests, such as that developed so patiently by Strong, are concerned with "characteristically long-time preferences," and sometimes with aspects of interest that might be regarded as "precious and intimate." The work of Bruner, Postman, and others has been concerned, in part, with the relationship between values and perception; they have studied a psychological function amenable to direct measurement in order to make inferences about the function of personal values in individual behavior. In so far as they speculate, as does Fromm, about the central function of values in human adjustment, they may be classed as philosophers; but when they systematically analyze verbatim protocols to determine how value variables influence behavior, as has Rogers, they are at least making a valiant attempt to apply scientific methods (as defined by Geiger) to value problems. These efforts may not represent an impressive attack on the all encompassing problem of ethics in human behavior, but they at least indicate a willingness of psychologists and other social scientists to concern themselves with problems that are a part of the fabric of ethics. I am inclined to believe that there is no particular reluctance on the part of social scientists to concern themselves with the study of values, except a natural reluctance to continue climbing the rungs of methodological ladders have given out.

My most direct experience with the problem of investigating values has

[2] Lundberg, George A., "Can Science Validate Ethics?", *American Association of University Professors, Bulletin*, 36: 261-275, 1950.

been in connection with the work of the Committee on Ethical Standards for Psychology of the *American Psychological Association*.[3] A brief summary of observations growing out of the work of this Committee may highlight methodological difficulties of a frontal and large-scale attack on ethical problems.

I recall being much impressed, in the early months of the work of the Committee, with the reception given the notion that we might use empirical methods to develop a statement of ethical standards for psychology. For a number of years, the Association had postponed the chore of getting out a code of ethics for the profession, largely, I venture, because no one felt very comfortable with the prospect of taking thought and drawing up a statement of standards for others to live by. When the Committee worked out a plan that was more in keeping with the methodological traditions of psychology, involving an empirical approach to the definition of ethical standards, the response of psychologists was enthusiastic. At the present time, well over a thousand psychologists have contributed to the enterprise in some fashion, suggesting a willingness on the part of psychologists to explore ethical problems when the method of attack is a congenial one.

Professor Geiger's observation that "objective, critical, and operational reports can be made about human decisions, and those reports, in turn, can themselves enter into the fabric of future decisions" summarizes well the methods and the objectives of the Committee in attempting a definition of ethical standards. We have asked psychologists to send in detailed descriptions of incidents in which psychologists made ethically critical decisions. These descriptions of incidents, over a thousand in number, provide the raw data for the work of the Committee. Our analysis of these data and our attempt to draw generalizations from them fall far short of best scientific methodology. All that we can claim, by way of being scientific about the matter, is a "temper of approach" which meets only the more general of Geiger's interpretations of the scientific method.

We have repeatedly been made aware of the limitation of counting techniques for identifying ethical problems in arriving at best solutions of value conflicts. Ballots fail to identify "best" ethical responses; the majority is not always right, if some other requirement, such as consistency of principles, is given much weight. Four persons might agree on a choice of being ethical, and a fifth person make an opposite choice which fits better into a pattern of principles, and which, indeed, might be recognized as the preferable choice by the voters who voted in opposition, if presented from a different frame of reference. We fall back on a process of successive approximations, starting first with concrete cases and moving through a tentative statement of principles involved, to revision and then more revision, which we hope will result in ethical standards having some substantial validity for American psychologists.

We have also recognized that to be valid, our statement of ethical stand-

[3] Hobbs, N., "The Development of a Code of Ethical Standards for Psychology," *The American Psychologist*, 3: 80–84, 1948.

ards must be more than a reporting of present practice; it must somehow express the aspirations of psychologists in their professional relationships. It is in estimating the direction and extent of this departure from present practice that our Committee has had its most difficult methodological problems. Here we fall back upon our judgment of the situation, a process that can hardly be called scientific. The only claim of this procedure to superiority over customary methods of defining ethical standards (as represented in the code of ethics of many professions) is that the judgments are usually made with reference to rather specific situations involving identifiable behaviors, rather than to more global issues.

Other limitations of method have made themselves apparent. We have no good estimate of our sampling error. Do psychologists who voluntarily participate in an effort to develop a code of ethics have representative (or "better" or "worse") value systems? We do not know, of course. Nor do we know how to assess the influence of our own value commitments on our selection of incidents, on our perception of their salient elements, or on our interpretation of the principles involved. We do not know to what extent our formulations will communicate ideas which are in no way an expression of our conscious intent; the personal biases of the small group of people who must first put something on paper to provide working materials for others' appraisal operate to an indeterminate degree. Furthermore, we should recognize that considerations not immediately relevant to ethics shape our formulations of ethical standards. Because of such concerns as the preserving of amicable interprofessional relationships, we must occasionally edit reported incidents and recommend principles since they might offend; we can admit that psychologists are occasionally unethical but we cannot sensibly make much of the fact that members of other professional groups also occasionally engage in unethical practices. Such sources of error, either in observation or in reporting, reduce considerably any claim that we might be tempted to make that our study of the ethical practices of one group of professional people is scientific.

From the outset, our Committee has worked on a big "If . . . . then . . . ." assumption. We have had as our goal not a descriptive study of current ethical practices among psychologists, but rather an investigation which would, by the operations involved therein, effect changes in the behavior of members of the professional group studied. *If* operations 1, 2, 3, etc., are carried on in developing the statement of ethical standards, *then* psychologists will be more ethical in their daily work; this has been a basic hypothesis in our work. The operations referred to entail procedures that have been developed most purposefully in social action research, such as obtaining widespread participation for appraisal of tentative formulations, and carrying out educational programs with newcomers to the field. The final operation will be the publishing of the derived statement of ethical standards; this last step we believe will be the least significant in the entire process, and the one that will contribute least to the variance in the "if-then" equation. Unfortunately, we will have no measurements of the extent

to which psychologists make more appropriate choices as a result of the project, and thus the "if-then" hypothesis will remain unchecked, except by the kinds of personal and unsystematic observations that interested psychologists will make.

The data we have at hand, comprising brief reports of a large number of incidents with ethical implications, may perhaps be analyzable in terms of several less ambitious "if-then" hypotheses. For instance, descriptions of incidents may be analyzed with reference to implied directions of loyalties. In reporting an incident, does the writer work from some implicit criterion for goodness of choice that presupposes a particular loyalty? Is science to be served, regardless of consequences to individuals or to society? Is concern for the individual paramount in determining choice? Or is the welfare of the individual assumed to be subordinate to some social good? Having identified such underlying assumptions, we may then seek to identify variables likely to be associated with the adoption of one of the assumptions. Some of the simpler of these variables that one would want to examine are age, sex, field of work (an experimental or a social psychologist?), religious inclinations, cultural background, and so on. More complex variables that one might expect to be associated with preference for a particular direction of loyalty would be such hard-to-define functions as personality, concept of self, or identification with certain social groups. Presumably too, one could make genotypic studies to throw light on disposition to operate from a particular frame of reference in making ethical choices. But these comments are largely speculative.

Considering the work of our Committee as something of a test case, albeit inadequate in many respects, for Professer Geiger's thesis, I believe one might venture the following conclusions. Psychologists have not been bothered by "the antique dualism between fact and value." They have tended to have an "of course" attitude toward the suggestion that empirical methods might be used to define ethical values for the profession. The main barriers to substantial accomplishment have been methodological. From one perspective, the APA's undertaking may be viewed as daring in conception, the first attempt in history (so far as I know) to apply empirical techniques to large scale value problems; from another perspective, however, we must exercise restraint in choice of adjectives. Our methods are gross and clumsy; the readings we obtain from the best of our instruments are only rough approximations; and no one will really know to what extent we have been successful.

WHEN DO WE START VALUE RESEARCH?

C. WEST CHURCHMAN

ONE HOPES, perhaps too optimistically, that there will be little or no objection to the main thesis of Professor Geiger's paper. As I understood him, he asserts that a discipline of human values can and should be established which will share the property of all scientific disciplines; namely, sound conclusions based on evidence and procedures that can be checked and controlled.

It seems to me that one has to be incredibly blind to deny that there are broad areas of human values that we do not understand today, but which we could readily understand if given the facilities to conduct the necessary research. I think, for example, of such problems as those of consumer values, of the true intentions of a community as opposed to their voting habits, of the value assumptions made by administrators or by courts, of the value decisions inherent in psychoanalytical procedures.[1]

The real problem is not *could* we, or *should* we, but when and how do we begin? We stand like a man on the brink of disaster, intellectually discussing whether he should take steps to save himself.

The typical academic objection to this remark will run as follows: "It's all very well to point out how useful value research would be, but you must understand that this is a relatively new and untried field, so that we have no way of competently examining a given proposal for such research, as we do in the better established fields. Indeed, much of value research does not seem feasible, because one is trying to get at the elusive quality of persons called their desires, interests and needs, and we don't know of any good technique for doing so."

Strange as it may seem, this kind of argument still gains wide acceptance among intellectuals, despite the evidence of the history of science that new fields of understanding always depart on an unchartered ocean and always run the danger of running off the edge of the world.

Given the two objectives: only to support research which is well established in its principles, or to support any kind of research which is obviously desperately needed, which objective are we to consider the most important? The really pertinent fact is that there are many people, well qualified in terms of their intellectual attainments, who have developed various plans for conducting research into human values. What could be clearer than the conclusions that *all* such individuals should be given the chance to try out their ideas? Surely our largess in support of research in the physical and medical sciences indicates that we are in no economy of scarcity that forces

[1] The reader may be interested in a recent report of a group of philosophers and social scientists which suggests some of the phases of values believed to need attention and to be susceptible to research. This is the report of the Committee on Cooperative Research in Values. Copies may be obtained at cost on written request to the author of this paper.

us to decide *now* whether A or B is right in his notions of value research. We have only to decide whether the man himself has given evidence of sound thinking, and then give him the support he needs to put his thinking to the test of research experience.

The point is that value research of all kinds needs funds. We must be realistic in the matter and realize that unless a man can devote all his energies in so complex a problem as human values, the results he obtains will prove nothing. Today most value research is done speculatively, in the idle hours of one's philosophizing. Even if speculation is the major part of value research (and I do not think it is) we need time to perform the necessary speculations. And if, as Professor Geiger suggests, the true determination of values depends upon experimental evidence, then we surely need the wherewithal to do the work.

To be specific, I am going to propose some advice to certain groups, which advice seems to me to follow from Professor Geiger's remarks and the statements I have just made.

First of all, a word of advice to the "fund-suppliers," be they foundations, government agencies, university administrators, or the like. Do *not* apply the criteria of "sound" and "well established" principles to proposals for value research, for such criteria *operationally* mean that the project must be reviewed for its competence by other thinkers. But in a field like that of human value research, there are bound to be bitter differences of opinion since so little is known in the way of positive evidence. Even the most honest of us cannot help injecting our own faiths into matters so little tried in controlled experience. So, a social scientist will look at a proposal for value research, will compare it with his own methods and thinking, will compare it with the methods of social research he thinks are good, and will be sure to object, either because he can't see the outcome or can't see the point.

Rather, the fund-supplier should apply only the criteria of intellectual competence to the man or men who make the proposal; there will be a gamble on a useless outcome perhaps, but who will deny the necessity of the gamble, especially when the cost of failure to support such research may be much more serious?

Further, the fund-supplier should *not* use the criterion of a definite and specific result at the end of some period of time, for again even the deepest student of human values cannot tell now what obstacles will appear, what new possibilities will be exposed.

Finally, in my listing of *don'ts*, I'll suggest that the fund-suppliers don't require that a man's language be clear and readily understandable in his statement of his proposal, for language only becomes clear when we begin to see the answers and understand them. Right now no one can be clear and precise when he talks about human values; such clarity is an expected outcome of value research itself.

There is one *do* I'd like to include for the fund-supplier. Do insist on and do supply the means for constant consultation on the part of the researcher with social scientists and others conducting value research, together

with the means for publishing tentative and final reports. The real danger is that a man's work will become an isolated construction, wonderful in his own mind, incomprehensible to any one else. The only way to prevent such an outcome is to organize meetings and informal discussions as frequently as feasible.

As for the rest of us, scientists, union members, administrators, and the like, there is one piece of advice. Now is the time for official statements from associations and groups on the subject of value research. Specifically, such documents should include a statement as to *why* value research is essential to the discipline or group in question. For example, does it make any difference to physics as a discipline whether we do or do not understand human values any better? Only the most hide-bound believer in the purity of mathematics and laboratory observation could answer this question in the negative, and even if he did so answer he would have dogmatically and uncritically asserted the value of mathematics and observation. The truth is that most scientists just don't answer the question because they don't want to think about it. Perhaps they can be made to think about it through the proposal of specific types of research in values to be endorsed by the associations.

Similar remarks apply to groups outside the academic fields. Such groups may not have the funds to conduct research itself, but they may well be able to apply the pressures for such research.

One final point; anyone who gives so much advice always appears to be the self-made angel of human wisdom who devaluates the efforts of others. No such intention is implied here, since the main issue is not who's to blame for no value research, or who's to blame for the dualism of fact and value, but rather, when do we begin?

REJOINDER

GEORGE R. GEIGER

BY USAGE, if not by definition, a "rejoinder" is an answering of objections, and, so, ordinarily negative, even defensive. It is encouraging to find that most of the papers in this symposium agree, in general, with the position of my original paper. It is equally encouraging to discover that it is to the limitations and extensions of my argument that most of the attention has been directed. (In this connection, I am deeply interested in Dr. Franz Alexander's opinion that the present topic has long since been outmoded. I can say only that I wish he were right!) But since a rejoinder needs to concentrate on disagreements, I have devoted most of my answer to them, especially to those of Professor Feigl. This has seemed the most effective way to employ the space allotted to me. So failure to mention most of the contributors is certainly no sign of neglect or of lack of appreciation. I am deeply indebted to them for their kind remarks and for the research data to which they have directed me, and I want to take this opportunity to thank all of them most cordially.

Turning to the objections of Professor Feigl, I find he is now a long way from the extreme left (or is it the extreme right?) of positivism, and is prepared to accept a great deal of scientific co-operation in the study of valuations and their context; but he refuses to take the last step, the only really significant step, and *make* evaluations on the basis of scientific knowledge. It would, of course, be impertinence on my part to try to account for this refusal, especially since Professor Feigl himself insists that it is based on logical grounds alone. He points out that logical clarity is essential, and proceeds to make a number of pronouncements which, I have to confess, seem elementary to the point of condescension. I was almost prepared to read that "there is a name for the illicit attempt to derive values from intelligence: it is called 'the naturalistic fallacy.' " Now, one should not cavil at logical clearness, even if the implication is that Dewey et al. (not to mention most of the contributors to the present number of this magazine) are obtuse in this matter. But I feel one is bound to protest, not at logical clarity, but at the substitution of grammar for argument. I find in Professor Feigl's paper a series of statements repeated now for a philosophic generation in precisely the same language, such as: It is one thing to *de*scribe . . . : it is another thing to *pre*scribe . . . The plausibility of the derivation of ethical principles from a factual account of needs and interests and the means toward their fulfillment is altogether specious. Such alleged derivations reveal themselves either as ***non sequiturs*** (for indicatives never imply imperatives), or as ***petitiones principii*** (for the interest assumed is already tantamount to a moral ideal.)

These are unequivocal statements indeed, but what supports them?

Are they the results of investigations (of the kind referred to in a number of papers here) or are they but corollaries of an interesting, though precious, definition of philosophy? In fact, are they not themselves (if one may borrow Professor Feigl's Latin) particularly egregious examples of a *petitio principii,* in that the conclusion is already decided, by definition, in the premises? The quotations from Professor Feigl's article seem to institutionalize a kind of word-magic in which certain counters have an implicit mystery, counters like "ought," "imperative," "prescription," and the like. Or, if "mystery" is too strong a word, then at least a kind of opaqueness, since they cannot be seen through except as "persuaders" or "motivators" and these—as noted above—do not depend upon human interests and needs and the means toward their fulfillment. So, an involuted word-analysis is to take the place of the possible contributions of present or future psychology and anthropology, and august terms like "ought," "good," "imperative" automatically arrest further (empirical) analysis of the value situation. I must repeat that I find no argument here, except reference to other analysts (such as Stevenson.) There is an antiseptic and sterile atmosphere throughout; and was it not Whitehead who said that we can't live by disinfectants alone?

Logical positivists and analysts—not that all of them can be described at once—seem to be in the position of a retreating army, always having to fall back upon defenses prepared in advance at the rear. There is the retreat (as in Stevenson) from "beliefs" to "attitudes" or (as in Feigl) from means to ends.[1] Then, there is always the last ditch defense which flatly asserts (as Professor Feigl does here) that science is value-free or value-neutral. (That this itself is a statement ascribing "value" to procedures of the scientific enterprise, procedures such as mathematics or empirical observation, must certainly be obvious to an analyst. Churchman and others have pointed this out on many occasions.) These defenses in depth are all constructed on a similar pattern and with one overall strategy as a guide—that of erecting a screen of language to forestall the advance of empirical activity, at least in this sector of evaluation. For instance, the positivist and analyst continue to repeat, in so many different ways, that values can never be reduced to facts. Yet, so far as I am aware, these philosophers have yet to take into account the truly revolutionary work in human behavior referred to by Dr. Franz Alexander, among others, or even to the work mentioned in my own original paper and in an earlier one.[2] These may not convince him that the fact-value divorce is untenable *on experimental grounds,* but they should make him aware of the extraordinary possibilities in a continuously developing science of individual and social behavior, or, at the very least, of the possibilities of a new science

[1] In *Readings in Philosophical Analysis* (New York, Appleton-Century-Crofts, 1949), Professor Feigl seems to take a much more relaxed and permissive attitude toward the ends-means dichotomy (cf. p. 24.)

[2] "A Note on the Naturalistic Fallacy," *Philosophy of Science,* Oct., 1949, pp. 336–342.

of value itself. To prejudge these movements in advance on grounds of definition alone is, if nothing worse, defeatism of a most irresponsible sort. Perhaps, however, the strategy of the analyst discloses that even the last defenses will some day be surrendered.

There is an unquestionable justification (as noted in my original paper) for Professor Feigl's distinction between "studying" and "making" evaluations. It is the obvious justification of division of labor and of handling first things first. There is vital heuristic and preparatory work in any kind of analysis, necessary if not sufficient. (But this is decidedly not the distinction he has in mind.) To distinguish between terms, to focus on the proper questions, even to isolate key words like "good" and its synonyms—all this is healthy propaedeutics. But to isolate is not to insulate, and certainly it is not to set off in some dim region of intuition the very subject-matter of a possible science of conduct. For the arbitrary and a priori refusal to permit empirical investigation in the area of valuation must inevitably lead to the celebration of moral intuition alone. True, Professor Feigl may not like this—even Stevenson doesn't. But to prohibit *ab initio* any attempt to derive the good from a knowledge of psychosocial behavior can have no other result than to turn over the dimension of the good to the private introspections of, say, British dons.

The same prohibition would also apply to "sociology of knowledge." I should not venture to accuse Professor Feigl of subscribing to the recent remark of an analyst to the effect that an understanding of Plato's philosophy would be hindered not advanced by knowing the historical and cultural setting of Plato's thought. But I fear he would subscribe to an equally characteristic remark such, for example, as that Dewey's *Logic* has little, if anything, to do with logic. My point here is simply that although Professor Feigl cheerfully repudiates as obsolete the traditional claims of ontological dualism, he is completely unable to see that his own logical dualism between knowledge and valuation smacks almost as much of the vestigial. Now, I quite agree with Professor Ayres that one can carry the dualistic thesis too far; but I agree with him even more enthusiastically when he insists that the historical conditions of cultural dualism must "be reckoned with in any definitive analysis of the value problem." I don't believe the positivist would reckon with it, nor do I believe he would even allow the use of his pre-empted word "analysis." Historical and situational analysis must always give way to logical. It is true that Professor Feigl refuses to separate pure from applied science, but this, I'm afraid, is a rather desperate last-ditch attempt to avoid the charge of obscurantism. For the one sure effect of his entire argument would be to preserve some phase of thought for "philosophy" and Logic alone, impervious to empirical attack. If the history of dualism in the West means anything at all, then this attempt of the analyst is right in line with previous dualisms, more sophisticated and suave to be sure, but just as surely an effect to distinguish what is empirically allowable and natural (knowledge and science) from what is not allowable and non-natural (valuation and ethics).

It perhaps will be regarded as an *argumentum ad hominem* to notice that the logical analyst becomes embarrassingly sensitive when his other-than-ethical naturalism is called into question. He belligerently professes to be a naturalist, as does Professor Feigl in his present contribution and elsewhere.[3] Yet he seems to stop his naturalism at the very point where alone it might be important not to stop. It is no trick to be a naturalist in metaphysics or even in epistemology. It deserves no credit to be a flaming radical—when confronting nineteenth century supernaturalism. To say (with both Feigl and Stevenson) that one has no desire for "the alleged ivory tower of 'pure analysis'," and that one wishes to give no comfort to the anti-naturalist, is to place one's self officially on the proper side. That is, it will indicate, in times of crisis such as these, that the philosopher does not simply worship the one true God, Tau-to-logy. Sincere this may be, but it still is the old business of trying to have one's cake and eat it, too. To be sophisticated and aloof in "philosophy" albeit a liberal in social affairs does not add up—except as one more expression of the traditional dualism which elevates thought (especially if it be esoteric) over action and almost guarantees an ideological schizophrenia. No one profits from this except the supernaturalist, whom the analyst professes to repudiate. Unless naturalism can extend its domain over the field of value, its other triumphs are insecure and, possibly, short-lived.

Even though this is a rejoinder and, so, deliberately "negative," it may be used as an opportunity for at least an elliptical reference to what some of us are trying to propose. Professor Ayres writes in his usual blunt way that what the instrumentalists are saying, and all they are saying, amounts to this: ". . . experience consists of a series (or continuum) of relationships all of which are knowable and understandable, and . . . knowing and understanding as much as one can is better than not doing so." The question-begging character of the word "better" is so deliberate here that even Professor Feigl would hesitate to call attention to it. Only, it is an attempt to answer not to beg the question. The answer puts its emphasis on process and continuity, the process of resolving conflicts and continuity of the tools which have been used most prosperously in that process. Now, is it "good" to solve problems? One answer, suggested in my paper, is that an affirmative response to this question may be a primitive notion, analyzable no further. But such an answer is always tantalizing and unsatisfactory. A more promising approach is to be found in the concept of a fully- integrated experience, the kind of experience Dewey has always tried to make us envisage, nowhere more eloquently than in *Art As Experience.* This is, of course, the culmination of anything called "instrumentalism," and the answer to irresponsible questions about "instrumental for what?" Dewey and others have tried to make abundantly clear that the only "ultimate" goal—to which science, reflective thinking, problem-solving, and the rest are "instrumental"—is the consummatory aspect of experience

---

[3] *Readings in Philosophical Analysis* (*op. cit.*); pp. 25–6; see also Stevenson, *ibid.*, p. 593.

most characteristically and genuinely expressed in the esthetic experience. To remove the frustrations, the truncations, the incompatibilities which prevent the consummation of experience, in other words, to solve problems —this is the criterion of creative intelligence. Now, in the context of solving problems, intelligent action is indeed "ultimate" but only in the sense of being "the last, the final or closing, thing we come upon in inquiry into inquiry." [4]

Thus, Professor Ayres is correct in arguing that it is better to solve problems intelligently than not to solve them or to "solve" them unintelligently. Here is an ongoing process—involving the whole technological continuum of scientific method—which provides a legitimate context for the derivation and selection of values. That there are other continuities beside that of problem-solving, we have already recognized. That there are irrational and non-rational aspects of experience, to which Professor Ayres and Dr. Alexander have called attention, must be given perhaps more consideration than some philosophers have thought necessary. And that there remains the whole matter of determining what a consummated experience is like—this, too, must be understood. But these are other aspects extending beyond our present interest, which is that of the problem-solving matrix of value.

The word "approach," used some lines above, must be emphasized here at the end. The work of even Dewey himself—as Churchman and Ackoff have recently pointed out [5]—has not provided the methodological details nor the quantitative handling which a science of values demands. Perhaps, as they suggest, no one of the social or psychological sciences may be in a position to develop further the problem of value and possibly a new discipline is indicated. The point is simply that the present argument has outlined, not an answer, but a way of approach, a method of attack on the vital problem of value. I hope it is an approach which can prove capable of enlisting the emotional commitments of both philosophers and scientists.

---

[4] Dewey, John in *The Philosophy of John Dewey* (Northwestern University Press, 1939) p. 594.

[5] In *Methods of Inquiry* (St. Louis, Educational Publishers, 1950), p. 531.

THE VALUING OF THE SOCIAL SCIENTIST

A Selective Summary in Process

KENNETH D. BENNE AND G. E. SWANSON

IN OUR INTRODUCTION to this number of the *Journal,* we tried to show that problems of valuing penetrate every aspect of the scientist's life as researcher, expert consultant, and citizen. We have before us now the considered judgments of the members of a symposium—a theologian, philosophers and social scientists—called to reflect upon these problems. As Editors, we are offering, by way of summary, our own reflections concerning the bearing of the symposium upon problems of valuing particularly as they focus for social scientists. What do the judgments of the symposium offer for the guidance of valuing? What issues concerning valuation does the symposium raise and clarify? What issues merit further study and discussion? Do the judgments of the symposium offer more help to social scientists in some of their roles than in others; in their long-range (strategic) choices than in their short-range (tactical) decisions, etc.? Since we see our summary hopefully as an aid to comparable reflections by other readers of these materials and to a continuing "symposium" by philosophers, scientists and theologians on "values and the social sciences," and not as a "definitive substitute" for these desirable eventualities, we have taken the liberties of being selective in our work and of raising more questions than we attempt to settle.

A. *Some Limitations of the Symposium*

Before commenting upon the accomplishments of the symposium, we would like to clarify certain limitations which we, as well, we are sure, as the contributors and readers, recognize in it. Limitations of space and time forced certain choices upon both the editors and contributors. Limitations of time forced us to give up the solicitation of comments from representatives of social science disciplines not adequately represented in the symposium—notably sociology, cultural anthropology, political science and law—after those from these fields who had originally accepted assignments found at a late date that it was impossible for them to carry through. Our efforts to get comments from non-American social scientists and from a scientist committed to Marxism proved abortive. We are sure that Professor Geiger and the other contributors also looked, and look, longingly at alternative lines of development which space and time limitations have excluded. Nevertheless, if it is taken as a principle of wise valuation that those who are valuing try to acquire clarity concerning the alternatives which their choices, in collaboration with circumstances, have left in the state of potentialities, as well as those which choice and circumstances have helped to actualize, there may be wisdom in pointing to three aspects of

the problem of valuation with which this symposium does not deal explicitly.

1. *Little is done in any of the contributions to specify clearly and comprehensively the "metaphysical" assumptions underlying the positions taken and defended with respect to valuation.* We mean by "metaphysical" in this context ("metascientific" might be a better term, if less traditional) what Geiger refers to as the "primitive" assumptions which underly and, at some "ultimate" level of generality, "make sense of" the principles and derivations which characterize any explicit point of view, in this case a point of view with respect to the possibilities, necessities and desirabilities of human choosing. There may well be symposium members, as well as readers, who would find such "metaphysical" elaborations useless, ostentatious and even meaningless. But, for some symposium members and readers, the "metaphysics" of a conception of valuation might well seem to make significant differences in the derivations drawn from the conception for the guidance of choice and conduct. However serious the omission may be, this symposium contributes little or nothing to a "metaphysics" of valuation.

2. *None of the contributors has chosen to orient his discussion of valuation directly to the principal ideological struggles, which, as politically organized, define the great power struggles of our century.* Whatever else such ideological struggles may involve, they do involve deeply divergent and unreconciled value assumptions. And the choices of the social scientist, at least as citizen and as expert consultant, if not so obviously and immediately as researcher, do have consequences for the areas of unresolved "public" choice demarcated by competing ideologies, just as these wider processes of public choice have consequences for his own processes of choosing.[1]

We shall note later that all of the contributions to this symposium seem to reject a "totalitarian" solution to value conflicts, a solution based on attempts to enforce politically a "partial solution" and to suppress public deliberation which challenges the "partial solution." In this sense, they are all "liberal" viewpoints. As such, each may be seeking a general scheme of valuing—a scheme with the virtue of applying to *all* valuing across the lines of divergent ideologies. This very generality means, however, that some issues, specific to particular current value and power conflicts, go untreated. If, for example, logical, psychological and social bases for reconciling deeply divergent outlooks are found, the problem still remains of effectively and persuasively communicating these mediating principles and conceptions to the parties to the conflict. In the case of Cattell at least, there is the suggestion that differences in interest and outlook within the world's population are desirable as sources of new and possibly "better"

[1] We are here using "ideology" to include the preferential viewpoints both of groups seeking the reconstruction and of those seeking the conservation of existing social and political institutions; not limiting it to the viewpoints of the latter group, as Mannheim does in his *Ideology and Utopia.*

insights and as a "laboratory" for the testing of varying ethical principles. But we are not told which differences and conflicts are tolerable and which intolerable; which differences and conflicts promise to serve the development of new, tested ethical principles and which ones threaten the very fabric of previously "tested" and institutionalized principles upon which the emergence and testing of new insights in some measure depend. Here, perhaps, our symposium reflects the dilemmas characteristic of traditional Western liberalism at the present time. At any rate, there is little explicit orientation of the various contributions to the most pressing ideological struggles of the 1950s.

3. *For the most part, the symposium gives little explicit attention to the collective aspects of processes of valuation.* (Wieman and Raup are exceptions to this observation.) For the most part, the "root metaphor" underlying the discussions of valuing seems to be "the person in the throes of individual decision." Processes such as communication, the construction and operation of group standards, and the reconstruction of social norms, tend to be only loosely related to processes of choice and decision. It may be that processes of "individual" and of "group" choice and valuation are equivalent, logically and psychologically, but it is generally true that the similarities or differences are not explicitly treated in this symposium.

B. *Toward a Science of Valuation*

The Strategic Deployment of Scientific Resources

The other symposium members unanimously second Geiger in his attack on those who would exclude values and valuing from scientific study. All members of the symposium accept Geiger's idea that the behavioral locus of valuation is in human "choice" and "preference." All agree that the choices and preferences of men can be studied scientifically. All seem to agree that such study is desirable and that its results would be useful to all human beings in making the choices which they must make. In most cases, Alexander being the principal exception, the needed scientific study of valuation seems to call for a refocusing of effort on the part of social scientists, a redeployment of scientific resources, a departure in some degree from current scientific prepossession with the study of non-valuative phases of human conduct or with inadequately conceived studies of valuation. If social scientists were to accept these interpretations and valuations, they might well ask the members of the symposium what strategy they should employ in redirecting scientific effort toward the development of a science of valuation. What should we do as social scientists in getting on with this long-range task?

1. *Barriers to Scientific Study of Valuation and Strategy Suggestions.* At this point, we seem to have less unanimity within our symposium. The strategy required by any human enterprise depends in part on the assessment of the *barriers* which need to be overcome in advancing the enterprise. And different members of this symposium see different barriers as crucial to an extension of a science of valuation.

Geiger seems to see the principal barriers residing in the persistent metaphysics of appearance and reality in Western thought and the institutionalized supports to this dichotomy which have developed in Western culture. This metaphysical dichotomy is engrained not alone within the thinking and practice of non-scientists. It is characteristic also of the assumptions of many scientists. The strategy called for by this assessment would certainly include the philosophic re-examination of these assumptions and the philosophic exorcising of the unlaid ghost of metaphysical dualism both by scientists and by non-scientists. In this task of re-orientation, social scientists, along with other men, must philosophize about the assumptions underlying current taboos against the scientific study of values. Presumably, the cooperation of philosophers in the process would be useful.

Ayres seems to make a similar diagnosis, but he is impressed by another duality which operates in human valuation. Men find value in the observance of ritualized mores, often more value than they find in the intelligent use of tools and knowledge. It is his counsel that social scientists themselves accept the value of knowledge, including knowledge of non-cognitive mores and sentiments, in making the decisions which they must make, and assert this value effectively in and to the community of non-scientists. Ayres, too, seems to believe that social scientists should accept philosophic advice, at least from some schools of philosophy.

On the other hand, Cattell sees major barriers to the scientific study of valuation in the dogmatism of theologians, philosophers and some humanists who have preempted the field of valuation for their own non-scientific or perhaps anti-scientific enterprises. The required strategy would seem to be for social scientists to declare their independence from these hampering disciplines and to advance the science of valuation through their own efforts.

Raup and Feigl seem to see an overwhelming pride in the claim of scientists that they can, by the exclusive use of their own methods, exhaust the study of human valuation. A barrier to sound progress in scientific studies of valuation seems to lie for them in scientists' and others' claiming too much for the method of science in such studies. The strategy required seems to involve a critical reassessment of the limitations of scientific method in valuation.[2]

For Alexander, no significant intellectual barrier to the extension of a science of valuation now exists. The human sciences, particularly psychoanalysis, biology and anthropology, are already on their way to the development of such a science. The strategy called for, on such a view, would seem to be to gain additional support, moral, intellectual and financial, for the kinds of studies now being undertaken.

Wieman sees a principal barrier to the development of a science of valuation in the persistence of an inadequate "interest" theory of value which he finds in Geiger and probably in much of current social science. His strategy calls for the development of educative techniques which will

[2] More will be said of this issue later.

reveal to valuers all of the determinants of valuation and thus furnish a basis for the valuation of interests and values based on interests. This demands further a conception of valuation which takes account of its inherent interpersonal and social character. He too finds studies which incorporate the required techniques and conception under way in current social science. It is interesting to note the differences between the studies which he cites as evidence of the beginnings of a social science of valuation and those which Alexander cites. A study of these differences might reveal important additional differences as to the probable and desirable character of a developed science of values.

Churchman, in what might be read as a view opposed to those of Alexander and Wieman, seems to find that "scientific" treatments of value are, up to the present time, mainly "speculative" and that they will remain so until financial support for extensive and varied experimentation directed to the solution of value problems is obtained.

Hobbs finds important barriers in the inherent complexities of processes of human choice and in the lack of adequately developed methodological tools for dealing with these complexities scientifically. The strategy on this view would seem to be to concentrate on the development of appropriate research tools and techniques.

Lippitt suggests a barrier in the current institutional separation of social scientists in the research phases of their work from the arena of public choices in social action and planning. The strategy, on this view, is to include action research on the agenda of social science, both to cultivate a relationship with the field of action which will make possible significant basic research on action-decisions and to advance the required re-education of social scientists, as researchers and as educators of the public, in scientific methods for dealing with choices.

This variety of assessments of the current state of scientific studies of valuation and of barriers to their further development seems to us to provide a significant agenda for further strategy-thinking by social scientists. Part of the variety is due to different, but by no means contradictory, emphases. If we are to advance the scientific study of valuation, we probably do need philosophic re-examination of our assumptions about knowledge, value and culture *and* intensive work on the development of appropriate and new research techniques *and* re-assessment of the virtues and limitations of current scientific methodology for such studies. We probably do need to assess the beginnings of a science of valuation in current social science *and* encourage varied investigations along new and unorthodox lines. But part of the variety is due to conceptions which are in opposition. These need to be pointed up as an aid to further joint deliberations by social scientists, possibly in company with philosophers, theologians and others.

2. *Is a NEW Science of Valuation Needed?* This question is important from the standpoint of research, since it raises the related questions of how much crossing of the lines of traditional social science disciplines is necessary and of how much departure from the kinds of questions raised for

research purposes in the traditional disciplines is indicated. Cattell implies that a new discipline of scientific ethics is in the offing. The findings of this new discipline will be used in the further development of traditional disciplines and in applied social science. But it will, nevertheless, be a new discipline. Churchman, too, states that a new discipline is needed. On the other hand, the implication of Alexander's paper is that the disciplines of biology, psychology, sociology, and anthropology are adequate to investigate problems of value pertinent to their domains. Not enough is said directly on this issue by any of the symposium members to make the reasons underlying the differences clear. We believe that further clarification and discussion of the issues involved, and of the related questions suggested above, would be highly profitable.

3. *Do Scientists Need the Help of Non-scientists in Studying Values?* We have noted already the different valuations put by different symposium members upon possible collaboration by social scientists with philosophers and theologians. Ayres, Geiger, Raup, and Feigl seem to imply, with whatever different emphases, that there is need for philosophic clarification and perhaps re-orientation in enrolling social scientists effectively in the study of valuations. The implication is, further, that scientists might well seek the advice of philosophers in intellectualizing their value problems and in assessing methodological requirements for dealing with them. Cattell seems to reject collaboration with philosophers and theologians as leading to no good results. It is generally admitted in the symposium that philosophers and theologians have traditionally "preëmpted" the field of values. They have had a long experience in formulating and discussing problems in that field. The question may be raised as to whether philosophers and theologians have learned enough about values in this experience that is relevant to further scientific study to make their collaboration useful to social scientists. Or, alternatively, has the experience of philosophers and theologians led to so much mis-learning on their part that scientists had better stake out the field of study for and by themselves? Perhaps the present symposium will serve as a case study for readers with respect to the possible fruitfulness of philosophic and scientific collaboration. It will, of course, not solve the difficult problems involved in learning to collaborate effectively in interdisciplinary research enterprises, if such collaboration is found to be desirable.

The unsettled question concerning the appropriateness of action research as an enterprise for social scientists can be raised in this same setting. Will social scientists learn or mis-learn in collaborating with action leaders and action groups as they try to use scientific methods in the making of choices and value judgments? Here again, Cattell defends a doctrine of "purity." Just as social scientists should avoid traffic with theologians and philosophers, they should avoid traffic with social action until they have worked out a science of ethics by and for themselves. Lippitt seems, on the other hand, to believe that social scientists can learn from action leaders, as well as teach them, if certain conditions of co-responsibility can be established and maintained in the relationship. At the least, he seems to argue, scientific studies will not be adequately permitted or supported in our culture

until the public comes widely to accept the values of scientific study in the management of their own non-scientific enterprises. At the best, the scientist can learn from those engaged in decision and action concerning the conditions and characteristics of adequate choice. Churchman emphasizes that, among non-scientists, at least the support of fund-suppliers and of those who influence fund-suppliers is needed to make the effective study of value-problems possible. Here again additional clarification and discussion of the issues involved are needed.

4. *How Far Can Scientific Method be Depended Upon in Making Wise Valuations?* It is with respect to the adequacy of scientific method as a guide in the making of wise choices that the symposium presents the sharpest divergence of views. Part of this divergence may lie in different interpretations of the meaning of "scientific method." But, even where agreement on the meaning to be given to "scientific method" exists, there seem to be different conceptions of its usefulness in settling value conflicts. It should be noted that there seems to be little or no dissent from the desirability of developing and using some systematic and empirical method of making choices, even where the adequacy of scientific method for that purpose is questioned.

In his essay, Geiger concluded that if values are synonymous with the outcomes of any process of choosing, the question becomes one of whether or not scientific method can be used as a means of making better choices. His answer was affirmative. Scientific method implies a process of free inquiry involving relevant data concerning the interests of the choosing individual, the alternatives confronting him, and the meanings of these alternatives in terms of their probable consequences, if acted upon, for himself and others concerned. Out of this process of free inquiry will come a decision based on interests, criticized, informed, reconstructed and chosen in terms of the meanings considered in the inquiry.

The questions raised concerning this viewpoint by three members of the symposium may be summarized in four paragraphs.

a. The position is incomplete because it does not specify the role of intelligence in revealing alternatives and new problems of choice outside the currently perceived needs and interests of the individual or group concerned. It does not see valuation as inherently and necessarily involving intercommunication. (Wieman raises this not as a question of the applicability of "scientific method" in processes of valuation but rather as a question concerning Geiger's interest theory of value and, by implication, a role for "scientific method" in processes of valuation which Geiger neglects.)

b. While scientific method is our best resource in producing descriptions of what exists and of the meanings of various alternative actions in terms of their probable consequences, it does not provide prescriptions of what *ought* to exist or of what actions we *ought* to find good. Put differently, the task of scientific method is to produce dependable cognitions but there are problems of choice which involve cognitively irremediable conflicts of value. (Feigl.)

c. Geiger's position is incomplete in failing to specify the need for inter-

personal communication (interpersuasion) in helping an individual to learn what areas of choice should concern him, what criteria are available or required for making optimally satisfying choices and what methods can be used to maximize the effectiveness of such interpersuasion in producing commonly desirable decisions. (Raup.)

d. Geiger's position uses as a basic "case" of valuation the solving of a problem in which the effort is to determine accurately some matter of descriptive fact. A somewhat different case of valuation is the solving of a problem in which a practical judgment is involved, an effort to produce a commonly desired and desirable policy or decision. Such problems do require accurate information for their adequate solution (the methodology appropriate to them includes, therefore, scientific method), but they require centrally the building of common sense norms prescribing required and desirable conduct (and the methodology must include, centrally, disciplined ways of determining commonly acceptable norms). (Raup.)

These challenges to Geiger's position raise but do not settle an issue of central importance for social scientists as well as for human beings generally. Geiger, Wieman, and Raup would all insist that wise choices require discipline in a publicly responsible and intersubjective method on the part of those choosing. (We do not include Feigl here because we are not certain whether he believes that a publicly responsible and intersubjective method of choosing is possible.) The question which these challenges raise is whether the discipline required in the making of wise choices is equivalent to discipline in the scientific method. Can and should scientists make the value judgments which they must make as researchers, consultants or citizens on the basis of the discipline they employ in solving a problem once it is *chosen* for scientific study? Should they, if they try to educate the public in the making of better choices, seek primarily to extend the discipline of scientific method in the public? Geiger would say "yes" to both questions. Wieman, with appropriate amendments to Geiger's theory of value, would also say "yes." Feigl seems to be saying to scientists that scientific method is inadequate to the making of value judgments, whether by scientists or non-scientists, though he does not propose a supplementary discipline. Raup would urge that scientists as well as non-scientists need to learn a discipline appropriate to the extension of consensus in areas of conflict with respect to appropriate policy, a discipline in which scientific method has a part, but only a part, to play. We believe that major decisions concerning the proper education and re-education of social scientists (and of other human beings as well) depend upon how this issue is resolved.

C. *Improving the "Tactical" Choices of the Social Scientist*

We have thus far emphasized the contributions of this symposium to the clarification of the "strategic" choices which the "profession" of social scientists needs to make in advancing the development of a science of values. We have found agreement in the symposium that the scientific study of values is possible. We have found also widespread agreement that more adequate scientific studies of valuation will make the processes of "tactical"

choice and decision by social scientists and other men better informed and less arbitrary, whatever other disciplines and resources may, according to some members of the symposium, still be necessary and desirable in making such choices. But most members of the symposium agree that the science of valuation is still in a highly primitive state of development. Meanwhile, the social scientist must make choices, as we tried to make clear in the introduction to this symposium, in his roles as researcher, consultant, and citizen. And he must make those choices without the help of a fully developed science of values and in the absence of any final policy decision on the part of the "profession" of social scientists (or the human race) about ultimate values. What has the symposium to offer the social scientist concerning the improvement of his "tactical" choices under these conditions?

1. *Choices are of the Better not of the Best.* It should be of some reassurance to social scientists confronted with choices if they can agree with most members of this symposium in forswearing the necessity of making final judgments about ultimate values. (Cattell may be an exception here.) Most members of the symposium seem to agree with Geiger that choice is always made within a situation with some finite number of alternatives. The situations of choice are typically marked by conflict as to which of the several finite alternatives is better. The parties to any given conflict do not differ with respect to *all* values. They do speak with different voices, urging different values, with respect to the unsettled aspects of the situation. The process of choice, whatever else it involves, requires the structuring of the problem so that the areas of agreement and disagreement are clarified and the disagreeing voices are allowed to speak to the decider. The process of choice is in some measure the answering, in terms of the best relevant information available and the widest possible sensitivity to the competing values at stake, of the differing voices in forming a judgment as to which of several alternatives seems "better." Judgment is always a venture. (It would be less a venture if scientific knowledge of relevant valuations were more copious than it is, but it would still be a venture.) It is a "better" venture if such conscious deliberation is attempted than if it is not.

This construction upon the process of choosing is best illustrated in this symposium by Lippitt's contribution. The various parties to the conflict concerning participation in "action research" are allowed to speak in his deliberations. The common ground is discovered; the alternatives explored and mapped. "Values" and "facts" are not held apart but are allowed to interplay. Difficulties in the decision, as it shapes up, are anticipated; possibilities of altering the decision, if certain eventualities develop, are considered. The colleagues to whom the scientist speaks about his problem may not agree that he has chosen the "better" course. But they and he know why he has chosen as he has. Communication is maintained and some common basis of evaluation of the results of his choice has been safeguarded. The foundations for some eventually common agreement, across present lines of disagreement on what is "better" in this kind of choice, have been laid.

2. *Increasing the Intersubjectivity of Our Choices.* Even if social scien-

tists accept that the goal of choosing is a "better" judgment of what is "better" in a finite situation rather than a certain and final judgment of what is "best" all around, they still have the problem of trying to increase the "intersubjectivity" of their judgments, where "intersubjectivity" can not, because of conflict, be assumed. Wieman would probably urge the social scientist to expect that there are alternatives and values other than those which his "interests" press upon him and other than those dictated by the "interests" of his opponents. Wieman would perhaps suggest also that the chooser take steps to find such alternatives through the fullest possible communication with others concerned in the choice—his colleagues and his critics. Raup might well urge further that the concerns of all the "communities" with a stake in the choice be considered and the representation of the widest "community" possible be sought in the value judgment upon which action is undertaken.

Hobbs offers a case example of a group of social scientists seeking to increase the intersubjectivity of their ethical standards through a search for consensus. Hobbs and his colleagues are faced with the need for developing policy criteria for guiding the value decisions of professional psychologists in their work. They have not been satisfied to accept norms which reflect merely the central tendencies in psychologists' current attitudes and practices with respect to value problems. The committee set itself the goal of developing a method which would involve the professional community of psychologists in becoming more sharply aware of "value situations" in their work and more committed to defining the "ethically better" in their choices. The method is one problem and a difficult one. But there seems to be another difficulty which Hobbs suggests. Pressing on them in their work is the wider social reality—a system of beliefs and supporting power that will ignore, reject, or support various criteria that the committee may propose, and with which any ethical code resulting from the committee's work must come to terms. How the committee will involve this wider social reality responsibility in developing a code acceptable to psychologists and the public alike is not made clear. But the necessity for finding ways of extending the "intersubjectivity" of their judgments into this wider community of concern—a requirement which Raup urges as a condition of validity in value judgment—is here apparent.

3. *If We Are Successfully to Reject the Totalitarian Solution*—In nearly all of the contributions to this symposium, there seems to be an implicit if not explicit rejection of the totalitarian solution to value conflict, as already noted. By implication at least, some degree of preferability of "democratic" solutions to value conflicts is affirmed. We might wish that the bases of preference for "democratic" values had been made more explicit. Perhaps that is properly the subject of another symposium. In it the interrelationships of similarity and difference between the values implicit in the scientific enterprise and the values of "democracy" might well be traced. We have suggested, in light of this symposium, that, pending the fuller development of a science of values, the chief reliance of social scientists in improving their

"tactical" choices is to improve their discipline of judging "the better" in finite situations marked by value conflict. We have suggested further that part of this discipline is skill in increasing, through intercommunication, the "intersubjectivity" of such value judgments. We would suggest that an aid to judging how far democratic values are required as elements in such a discipline is to translate democratic values into norms of a methodology for resolving value conflicts.[3] Perhaps the next symposium on values and the social sciences might begin at this point.

[3] Readers may wish to examine an initial attempt to formulate democratic norms in methodological terms. See Benne, Kenneth D. "Democratic Ethics and Human Engineering," *Progressive Education,* May, 1949.

THE EDITORS

Kenneth D. Benne is Professor of Education, University of Illinois. His field of specialization is the philosophy of education and he is President of the Philosophy of Education Society and a member of the Board of Editors of *Educational Theory*. As a staff member of the National Training Laboratory in Group Development, he has been concerned with studying the ethical and methodological foundations of applied social science. He is author of *A Conception of Authority* and co-author of *The Improvement of Practical Intelligence*.

G. E. Swanson is Assistant Professor of Sociology and Research Associate in the Research Center for Group Dynamics, University of Michigan. He received his B.A. and M.A. degrees from the University of Pittsburgh and his Ph.D. degree from the University of Chicago. He has taught sociology at Boston University, Indiana University, the University of Pittsburgh, and the University of Chicago. His research interests center on typologies of problem situations as determinants of social structure and functioning.

THE CONTRIBUTORS

George R. Geiger, now Professor of Philosophy, Antioch College, has also taught at the Universities of Wisconsin, Illinois, and Missouri. He is the author of a number of books in social philosophy, the most recent being *Philosophy and the Social Order.* At present he is national secretary-treasurer of the American Philosophical Association.

Clarence E. Ayres is Professor of Economics, University of Texas. Past President of the Southwestern Social Science Association, his major scholarly contributions have been made in the study of economic theory and history, institutional economics, and problems of contemporary economic order. He is author of a number of books including *The Theory of Economic Progress* and *The Divine Right of Capital.*

Raymond B. Cattell, Ph.D., D.Sc., University of London. Formerly G. Stanley Hall Professor, Clark University; Lecturer, Department of Psychology, Harvard University; and now Research Professor of Psychology, University of Illinois. Author of ten books in the field of psychology, mainly in personality theory and measurement and in social psychology. His research has been concerned principally with determination of personality factors in terms of objective measurements, the experimental study of small groups, and the quantification of national culture patterns for the purpose of investigating cultural dynamics.

Franz Alexander, M.D. Director, Chicago Institute for Psychoanalysis; Clinical Professor of Psychiatry, University of Illinois; consultant, National Advisory Mental Health Council; author: *The Criminal, The Judge and the Public* (with Staub, Hugo), *Medical Value of Psychoanalysis, Roots of Crime* (with Healy, Wm.), *Our Age of Unreason, Psychoanalytic Therapy* (with French, Thomas M., *et al*), *Studies in Psychosomatic Medicine* (with French, Thomas M., *et al*), *Fundamentals of Psychoanalysis, Psychosomatic Medicine.*

Herbert Feigl, Ph.D., University of Vienna, 1927. Member of the Vienna Circle of Logical Positivists, 1925–1930. Rockefeller Fellow, Harvard, 1930–31 (and 1940). Taught at University of Iowa, 1931–40, and at the University of Minnesota (Professor of Philosophy) from 1941 —.

Henry N. Wieman, Professor emeritus, University of Chicago Theological Faculties is a student of the Philosophy of Religion. He is a past president of the Theological Society and has been active also in the American Philosophical Association. He has made special studies of problems of value as related to religious experience and to science. His most recent book is *The Source of Human Good.*

R. Bruce Raup is Professor of Education, Teachers College, Columbia University. His field of specialization is the Philosophy of Education and he is a past president of the Philosophy of Education Society. He is author of several books and numerous articles in his field. His most recently pub-

lished book is *The Improvement of Practical Intelligence,* of which he is a co-author.

Ronald Lippitt is Associate Professor of Sociology and Psychology, University of Michigan, and a Research Program Director in the Research Center for Group Dynamics. As a member of the Inter-University staff of the National Training Laboratory in Group Development, he is engaged in the study of the processes of application of scientific knowledge from the "human behavior sciences." His most recent book is *Training in Community Relations.*

Nicholas Hobbs is chairman of the Committee on Ethical Standards of the American Psychological Association. Until recently he was an Associate Professor of Education, Teachers College, Columbia University, and he is now Chairman of the Department of Psychology, Louisiana State University.

C. West Churchman is a member of the Philosophy Department at Wayne University. He has taught at the University of Pennsylvania and at Bryn Mawr College. He is Editor of *Philosophy of Science,* a quarterly journal, and Secretary-Treasurer of the Philosophy of Science Association. His major scholarly interests are in scientific method, theory of statistical inference, and theory of values. He is author of *The Theory of Experimental Inference* and co-author of a recent book, *Methods of Inquiry.*

Previous Issues

Yearly subscription (4 issues)	$ 3.00
Group subscriptions (to single address)	
10 or more	2.50
20 or more	2.00
Back issues	
single copy	1.00
per dozen	10.00
twenty-five copies	15.50
Supplement issues	.50

Volume I: 1945 (limited quantities available)

No. 2 Racial and Religious Prejudice in Everyday Living (Part II) Gene Weltfish, Special Editor.

No. 4 The Psychological Problems of Bureaucracy. Goodwin Watson, Special Editor.

Volume II: 1946

No. 1 Conflict and Cooperation in Industry. John French, Arthur Kornhauser, Alfred Marrow, Special Editors.

No. 2 Measuring Public Attitudes, Angus Campell, Special Editor.

No. 3 American Military Occupation: A test for Democracy, Waldemar A. Neilsen, Special Editor.

No. 4 Action and Research: A Challenge, David Krech, Special Editor.

Volume III: 1947

No. 1 Toward a Common Ground—International Social Science, Jerome S. Bruner, Special Editor.

No. 2 Social Diagnosis and Social Therapy, Elliott Jaques, Special Editor.

No. 3 The Mass Media: Content, Function, and Measurement, Franklin Fearing, Special Editor.

No. 4 Social Research in Political Decision, L. M. Hanks, Jr., Special Editor.

Volume IV: 1948

No. 1 Citizen Participation in World Affairs: Problems and Possibilities. Eleanor Maccoby and Ben Willerman, Special Editors.

No. 2 Dynamics of the Discussion Group. Leland Bradford and John R. P. French, Jr., Special Editors.

No. 3 The Consultant Role and Organized Leadership. Staff, Industrial Relations Section, M.I.T., Special Editors.

No. 4 The Social Psychology of Physical Diability. Lee Meyerson, Special Editor.

Volume V: 1949

No. 1 Participation, Culture, and Personality. Frank Riesman, Jr. and S. M. Miller, Special Editors.

No. 2 Community Self-Surveys: An Approach to Social Change. Claire Selltiz and Margot Haas Wormser, Special Editors.

No. 3 Consistency and Inconsistency in Intergroup Relations. Isidor Chein, Morton Deutsch, Herbert Hyman, and Marie Jahoda, Special Editors.

No. 4 Social Values and Personality Development. Max L. Hutt and D. R. Miller, Special Editors.

Volume VI: 1950

No. 1 Community Service and Social Research—Group Psychotherapy in a Church Program. Hubert Coffey, Mervin Freedman, Timothy Leary, Abel Ossorio, Issue Authors.

No. 2 Educational Dynamics: Theory and Research. Herbert A. Thelen, Issue Author.

No. 3 Professional Women in Modern Society, May V. Seagoe, Editor.

No. 4. Values and the Social Scientist, Kenneth Benne and G. E. Swanson, editors.

Supplement Series

No. 1 Social Responsibility. George Brock Chisholm.

No. 2 Social Science and the Atomic Crisis. Hornell Hart.

No. 3 The Psychology of Social Learning. Edward C. Tolman.

No. 4. Prejudice: A Problem in Psychological and Social Causation. Gordon W. Allport.

ORDER FORM — PREVIOUS ISSUES

The Journal of Social Issues
Association Press
291 Broadway, New York 7, N.Y.

Name..... Street.....

City and State.....

Organization or Occupation.....

Please send me the following previous issues of the Journal of Social Issues:

TOTAL ENCLOSED: $.....

Volume I	Volume II	Volume III	Volume IV	Volume V
No. 1.....copies	No. 1.....copies	No. 1.....copies	No. 1.....copies	No. 1.....copies
No. 2.....copies	No. 2.....copies	No. 2.....copies	No. 2.....copies	No. 2.....copies
No. 3.....copies	No. 3.....copies	No. 3.....copies	No. 3.....copies	No. 3.....copies
No. 4.....copies	No. 4.....copies	No. 4.....copies	No. 4.....copies	No. 4.....copies

The Journal of Social Issues Is for You

If you are a social scientist interested in the integration of the social sciences, and their application to the solution of social problems.

If you are a professional educator, group worker, personnel specialist, public administrator, mental hygienist, or other applied social scientist interested in keeping oriented to the growing edge of social research as applied to the problems of your profession.

If you are a citizen-leader faced with the problems of productive leadership in your organization or institution or community, and want to get all the help you can from current research on techniques of leadership, discussion methed, use of program survey methods, etc.

A limited supply of copies of most numbers of the previous five volumes are still available. Each number was prepared by a team of collaborating social scientists and centers on one special topic. For the titles which are now available see the list on the other side of this page.

New subscriptions for the four numbers of Volume VI can be secured by using the blank below. Future numbers will focus on such problems as "the social psychology of housing," and "values and the social science practitioner."

ORDER FORM — NEW SUBSCRIPTION

The Journal of Social Issues
Association Press
291 Broadway, New York 7, N.Y.

Subscriber..... Street Address.....

City and State.....

Organization or Occupation.....

Please begin this subscription to the Journal of Social Issues with Volume VII, No. 1, for which I enclose $3.00.

(If this is a gift) Please send gift card with donor's name as follows:

.....

.....